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ORIGINAL ARTICLES.

THE INFLUENCE OF PARTURIENT LESIONS OF THE UTERUS AND VAGINA, IN THE CAUSATION OF PUERPERAL INSANITY.

Read in the Section of Obstetrics and Diseases of Women, at the Forty-third Annual Meeting of the American Medical Association, held at Detroit, Mich., June, 1892.

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I venture to report the following cases as a contribution to the etiology and treatment of puerperal insanity:

Case 1.—Mrs. A. T. White, aged 33 years, has been twice married, first at the age of 17 years. Of this marriage one child was born. Her husband died two and a half years after, and after remaining a widow four and a half years she married her present husband, who is a minister. There is no family history of insanity. In 1882, three days after the birth of her second child she had an attack of puerperal insanity, maniacal in character, which lasted five months. She remained well until October, 1886, and was then again attacked with acute mania. After this had continued ten weeks she was admitted to the Maryland Hospital for the Insane. She was very much excited, violent toward her husband and others with whom she came in contact. She was extremely profane and obscene, irritable, morose and disposed to fight on the least provocation. She soiled her clothing, bed and room and was a source of great trouble to the attendants. A pleasant "good morning" addressed to her by the physician on passing through the ward was generally the signal for a volley of obscenity and profanity. She sometimes acted as if she had hallucinations of hearing, but on account of her ill-temper, no clear history of hallucinations or delusions could be obtained. She did not improve, but showed a progressive tendency toward dementia. Her menstrual periods were attended by an exacerbation of symptoms. She was always more violent at her periods. An examination was made of the pelvic organs last September, and the following conditions found to exist: The perineum was torn down to the sphincter ani, causing the vulvar opening to gape widely. The cervix uteri was lacerated to the vaginal insertion on the left side and to a slighter degree on the right. There was decided intra-pelvic induration on the side of the uterus. Believing that these abnormal conditions, together with the evident unfavorable influence of the menstrual periods, justified the induction of the menopause, I removed the uterine appendages on October 6, 1891. The operation was performed under aseptic conditions, all instruments and dressings being sterilized by steam and hot distilled water used both for cleansing the hands, surface of the patient's body and irrigation of the abdominal cavity. No chemical antiseptics or disinfectants were used. No drainage.

The right ovary was cystic and firmly adherent in Douglas' cul de sac. Left tube tortuous, and broad ligament thickened and congested. The abdominal cavity was irrigated until the water returned clear. Five deep and two superficial silk-worm gut sutures were employed to close the incision. Anæsthetic used, A. C. E. mixture. Patient recovered well from the operation. Sutures were all removed on the seventh day and the wound found firmly united.

Two months after the operation, the patient had shown considerable mental improvement. She began to take an interest in books, pictures, flowers, etc. While her attempts at conversation were disconnected she dwelt more on pleasant themes, and her former violence of speech had almost entirely left her. After Christmas she began writing letters to her husband, making inquiries of her children and expressing much affection for them. This she had not done for over five years. She continued to improve, up to a certain point, and at her husband's visits, she received him affectionately, but quietly. While memory of past events and love for her husband and children seemed to return gradually, there was still a lack of coördination of thought, and this has not further improved. The brain disorganization (physical basis of dementia) had probably progressed too far to be restored even approximately to the normal. At the time of writing, eight months after the removal of the appendages the patient is quiet and cheerful, although relapsing into profanity when irritated. She no longer fights, or soils her bedding, room or clothing. She dresses and undresses herself, makes her bed, sweeps her room and waters the flowers and plants on the ward. She is not restored mentally, probably never will be; indeed, is likely, I think, to pass deeper into dementia. But, from a violent, excited, noisy and dirty patient, she has improved so much as to allow her to be kept on the quietest ward in the hospital, and this gain may, I think, be largely, if not entirely ascribed to the removal of the uterine appendages. I may say that I subsequently sewed up the lacerated cervix and restored the vaginal outlet by Emmett's procedure, without any appreciable effect upon the patient's mental condition.

Case 2.—Caroline A., white, aged 39 years, married fifteen years and the mother of seven children. Last child was born in April, 1887, four months before her admission to the hospital. No history of insanity in the family. Four weeks after the birth of her last child she suddenly developed delusions of persecution; claimed that some one was after her and trying to kill her. Her language became very profane and vulgar. She at one time made a violent attack upon her mother. She was one of the most troublesome and destructive patients in the hospital. She would strip herself on the ward, attack the attendants and the other patients, use the most obscene language, break the furniture, dig the plaster out of the wall of her room, soil her clothes, bed and room, jump at and hug any man coming within her reach and make herself generally disagreeable to her surroundings. She was always worse during her menstrual periods, and at these times was kept secluded in her room on account of her tendency to strip herself.

Vaginal examination showed a moderate perineal tear, but a deep bilateral laceration with eversion and erosion of the cervix, and enlarged uterus. Pelvic induration of moderate degree in Douglas' cul de sac. Abdominal section with removal of the uterine appendages was done on December 15, 1891. Tubes on both sides were thickened, congested and convoluted. Left ovary adherent. Small cyst in left broad ligament. No irrigation. No drainage. Patient recovered well from operation and sutures removed on the seventh day. Incision firmly united.

The patient seems to be slowly recovering a part of her mental faculties. She has become cleanly in habits and no longer indulges in her former vulgarity. The day before this present writing, she received a visit from one of her daughters and her youngest child and received them with every demonstration of affection. Her conversation is not connected, but it is now neither violent nor offensive. She sleeps in a dormitory with six other patients, eats in the ward dining room, keeps herself neat and clean, and is industrious in the use of the needle. Barring the non-restoration of her mental faculties, there has been a complete transformation in the habits, acts and speech of this patient.

Case 3.—Mary L. B., age 37 years, white, married thirteen years, and mother of six children, the youngest four months old at the time of her admission to the hospital. The family history is bad, mother being at one time insane, and her father very intemperate. She was admitted to the hospital May 16, 1890. She had one previous attack of insanity ten

years before the present attack, but it is not certain whether it was connected with the birth of any of her children. She had delusions and hallucinations. She was never very violent, but was talkative, exalted, and would strip herself on the ward. She was very much run down when brought to the hospital, and gained strength very slowly under stimulants and nutritious diet.

During her menstrual periods she became exalted and evidently had increase of sexual excitement. Her face was flushed and she would try to get near to, and touch the physician passing through the ward. At other times she was quiet and unobtrusive, but evidently under the influence of her delusions.

Examination disclosed bilateral laceration of the cervix with thickening of the posterior lip. There was an inflammatory induration on the left side of the uterus, which was very sensitive to pressure.

On November 25, 1891, the uterine appendages were removed. Left ovary adherent and tube thickened and convoluted. Irrigation. No drainage.

Patient recovered without a bad symptom. Stitches removed on the seventh day, and incision found firmly united.

In this patient delusions of personality continued for several weeks after the operation, but gradually faded away. Her conversation became connected and rational and in two months after the operation her mental faculties seemed to be completely restored.

Her climacteric symptoms, headache, backache, constipation and nervousness, were especially severe, but at this writing, over six months after the operation, their severity is lessened, and she is more comfortable. Her mental condition is completely restored to the normal, and as soon as the symptoms of the menopause above mentioned disappear, I shall discharge her from the hospital as recovered.

Case 4.—Mrs. Fannie L. C., aged 28, white, married and mother of three children. No hereditary history of insanity. Eight days after the birth of her first child she became insane, the mental disturbance lasting two weeks. Seven months after the birth of her second child she had another attack which lasted fifteen weeks. A third attack began a year after the birth of her last child. Three days after this outbreak (on December 28, 1891) she was admitted to the Maryland Hospital for the Insane. She was excited, but very weak. Her language was shocking in its profanity and obscenity. Sexual excitement was pronounced. For several weeks her pulse was so weak and rapid, that at times her life was despaired of. She was kept in bed and fed every two hours with milk, eggs and brandy. Digitalis was given to keep up the force of the heart. Her mental condition did not show any signs of improvement with returning strength. An examination under anæsthesia disclosed a deeply ruptured perineum with gaping vaginal entrance, lacerated cervix and prolapse of the right ovary.

On March 9, 1892, the uterine appendages were removed. No adhesions were found. Both ovaries were very much enlarged, being at least three times the normal size.

On the day previous to the operation, this patient was cross, profane and obscene in her language. Within two hours after the operation, as I entered her room, she burst into tears, asked me to forgive her for the ugly language she had used toward me and the assistant physicians and attendants, and acted in an entirely rational manner. She recovered well from the effects of the operation, but on the eighth day after the operation, and the day after removal of the sutures, the evening temperature ran up to 102.4°, and on examination a mural abscess was discovered, which discharged freely through the stitch holes for about two weeks. In spite of this, however, her progress toward recovery, both physical and mental, was uninterrupted, and she was discharged, well, on May 8, two months after the operation.

It will be noted that in each of these cases there was some lesion of the genital passage dating from parturition. In three out of the four cases there was likewise inflammatory adhesion and other morbid changes in the ovaries and tubes. In the fourth case there was great enlargement of the ovaries, with prolapse of one of these organs, subjecting it to more or less constant irritation.

Do not these cases force us to the conclusion that the etiology of puerperal insanity is to be sought, not in the brain itself but in pathological conditions of the sexual organs—conditions which, in the first place, favor the absorption of septic materials, and which, secondarily, leave behind them a source of irritation, to which such sensitive organs as the ovaries cannot be constantly subjected without deleterious effects upon the organism.

I point with some satisfaction to the results obtained from the radical treatment adopted. Out of four cases, two are entirely recovered. The other two were cases already subjects of brain enfeeblement, in whom no recovery could be hoped for, much less expected. However, the transformation of these women from dirty, noisy, destructive and obscene viragoes to persons who can be tolerated upon the quietest ward of the hospital is a distinct gain to the institution and unquestionably to the patients themselves. In cases such as these, I am sure no objection based upon the "unsexing" of the patients can hold. My experience inclines me to the opinion that there are other reasons besides a pathological condition of the pelvic organs, justifying the removal of the ovaries and tubes in insane women, but when there is actual local disease demanding the operation without regard to the mental condition, a physician having the good of his patient at heart, dare not hesitate.

Time is lacking to review here the unsatisfactory theories that have been propounded to account for the origin of puerperal insanity. I offer here no theory, but submit the cases which I believe justify the following conclusions:

1. Puerperal insanity is, in at least the large majority of cases, an infection psychosis.

2. Without rejecting the influence of other factors such as heredity, anæmia, exhaustion, mental shock and distress, careful observation will show that few cases of puerperal insanity occur without preceding or coincident puerperal infection.

The reasons for this opinion may be briefly summed up as follows:

1. Puerperal insanity occurs in the great majority of cases within the first ten days after delivery—about one-half in the first five days—the same period during which puerperal infection usually occurs.

2. It is usually accompanied by elevation of temperature and other evidences of febrile disturbance.

3. The clinical form in which puerperal insanity manifests itself is, in the majority of cases, that of acute, delirious, or confusional mania. Depressive states are rare except as secondary forms. In other words, the most frequent condition is one most closely resembling febrile delirium.

4. The death-rate is much higher than in simple mania. Death occurs from exhaustion, usually with high temperature and rapid pulse.

5. Post-mortem examinations, though apparently infrequent in these cases, have shown grave involvement of the pelvic viscera.

6. Examinations of the pelvic organs during life show lacerations of the perineum and cervix uteri (facile channels of infection in the puerperal woman). As secondary conditions are found intra-pelvic (peritoneal) inflammations, and consequent abnormal locations, fixations and congestions of the uterus, tubes and ovaries.

7. The results of operations seem to show that removal of local sources of irritation increases the chances of recovery from the mental disease.

PUERPERAL HYSTERIA (INSANITY?).

Read in the Section of Obstetrics and Diseases of Women, at the Forty-third Annual Meeting of the American Medical Association, held at Detroit, Mich., June, 1892.

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While the alienist, the surgeon and the gynecologist have long recognized the importance of hysteria in its counterfeit presentments of various morbid conditions of the human body, the obstetrician has been exceedingly backward in connecting neuromimetic phenomena with the disorders incident to pregnancy and the lying-in state.

Indeed, a careful review of obstetrical literature for the past few years, fails to discover more than a half dozen or so articles in which this important subject has been at all considered, while the text-books on midwifery contain little or nothing upon this point, and nowhere have I been able to find a word written in regard to hysterical mental conditions developing during labor or the puerperium. Yet I am quite sure that manifestations of this nature cannot be so very infrequently met with, the fact probably being that they are not recognized as such, but are put down as simple cases of puerperal mania. It seems to be the general impression that the insanity of childbed is a mental disorder peculiar to lying-in women, differing essentially in its manifestations from insanity developing under other conditions.

The careful observation of a considerable number of such cases, and the study of their histories as noted in asylum case books, convinces me that this is not so, and I do not believe that even the most expert alienist could, without a previous knowledge of the case, point out a single patient suffering from this disorder from a ward full of insane women non-puerperal.

The fact that a patient becomes mentally sick during the act of parturition or later, does not go to prove that the form of psychoses from which she suffers differs in any respect from mental derangement appearing at other times.

The classification of such cases into melancholia, mania and, rarely, katonia, is, it seems to me, more or less arbitrary and incorrect, since few cases, excepting, perhaps, the very acute septic manias which speedily terminate fatally, but display sooner or later, and generally repeatedly, both of the two first mentioned symptoms. The psychic manifestations, therefore, during the course of the disease, should be recognized and spoken of only as symptoms, not forms, of the brain disorder. It is not my purpose, however, to dwell upon this somewhat mooted question. In the present communication I simply desire to call attention to a symptom of insanity, a condition which might be termed a functional psychosis, and not generally recognized. Patients afflicted with this hysterical psychosis, invariably present a history of ante-partum hysteria. The attack develops suddenly either during labor or soon after, continues for a few days, generally less than a week, and as suddenly disappears, leaving the intellect clear and unimpaired.

During the whole period of the attack the bodily functions continue normal, the lochia are not diminished, and there is, as a rule, no fever.

The transition from sanity to insanity, and the recovery from the mental derangement, is generally so abrupt that the symptoms appear and disappear, to adopt the language of Levingstein-Schlegel, "as by a blow."

It has been my fortune to observe several of these cases, one of them about two years ago at the Detroit Woman's Hospital, and another more recently, the only one of which I have adequate notes, in my private practice. The history of this case will illustrate the point which I desire to make:

Mrs. S., a large, robust-looking woman of 24, passed through her first pregnancy without particular disturbance, excepting that at times she was nervous and mildly hysterical—a condition which had existed for some years—and also suffered from obstinate constipation. As far as I have been able to learn, the family history is good. A married sister is said to have been flighty after the birth of her child.

September 2, at 4:20 A.M., I delivered the patient with forceps of an 8-lb. girl. The labor had been comparatively easy, but toward the end of the second stage, the pains diminished in frequency and force, and finally became so feeble as to necessitate the use of instruments, just as the head had reached the perineum.

As is my custom, the patient was given a small amount of chloroform to inhale during the pains of the second stage, and complete anæsthesia was induced for a moment when the forceps were applied and the head delivered.

There was a slight tear of the perineum beyond the fourchette, but not sufficient to necessitate suturing. The blood loss was slight.

The patient came out of the anæsthetic well, was bright and cheerful, suffered no pain, and expressed her gratification that the trying ordeal was safely passed. I left her at 7:30 A.M., quiet and comfortable.

At 8 o'clock, half an hour later, her husband, who had been out of the city, and whose absence during the labor had been a source of worry to the patient, returned, but she failed to recognize either him or, for the first time, her mother and the nurse, both of whom had been present with her from the onset of labor. She also quickly developed hallucinations of sight, became voluble and incoherent in her language, talked of a crowd of people about her and of boats, complained of pain in the back of the neck—over which she held her hand, and asked to have her husband brought to her although he was then standing within a few feet of the bed. During the day she slept at intervals, was quiet, pleasant and obedient, and took her nourishment without objection. The pulse was 87, the temperature 98.4. The lochial discharge was normal, and uterine and abdominal tenderness absolutely wanting. The catheter was used three times during the day on account of slight tumefaction of the parts, with probably, voluntary retention of the urine.

September 3. Had a good night. Condition much the same as yesterday. When her baby is brought to her she looks at it with interest and then turns away. Asked if she is not pleased with her child she smiles and observes that "it jumps." Pulse and temperature normal.

September 4. Slept well last night. Hallucinations less marked. Recognizes the doctor, and calls him the "man", but cannot recollect his name. Asks him to come back soon and to bring her husband with him. Bodily condition normal; voluntary micturition.

September 5. Mental condition somewhat improved, although the hallucinations remain and she is still incoherent. Says that she feels sore all over. The pain in the back of the neck still continues but is evidently less severe. When asked a question which she does not desire to answer she laughs and says "huh". Pulse, temperature and excretions normal; no abdominal tenderness. She is much pleased with a bouquet which her husband brought her, fondles the flowers and places them on her bosom, although she still persists in refusing to recognize the giver.

September 6. Slept well. The urine which has been plentiful, is now passed voluntarily and in considerable quantities. During the forenoon the mental symptoms continued as before, but at 6 P.M., while her husband was sitting by the bedside, her mind suddenly cleared, the hallucinations vanished not to return and she appeared as strong and well mentally, as she had been and was physically. She had, ap-

parently, no memory of what had occurred since the labor. The nape ache had disappeared, and only a slight pain in the sacral region was complained of. From this time on the case progressed favorably and there was no return of the mental trouble.

Remarks.—In spite of the fact that so distinguished an observer as Levingstein-Schlegel, is of the opinion that a psychosis with hallucinations developing during the first weeks of the puerperium, in the absence of other (non-puerperal) infectious disease and without preceding eclampsia, is due to puerperal infection, even though a careful physical examination fails to demonstrate the presence of fever or other somatic symptom, I think that septic infection may be ruled out in this case. The only other condition with which the mental manifestation exhibited in Mrs. S's case should be confounded is that known as Transitory Frenzy (Transitory Mania, T. Melancholia), but as the history of the case shows a marked difference in the symptoms displayed from those seen in the latter condition, I think that I am quite justified in excluding this and ascribing the psychic disturbance to hysteria.

The reason why this condition is not mentioned in works on mental diseases, is because it never comes under the observation of the alienist, being of short duration, and always cared for at home by the family physician, who, in all probability, also fails to recognize the nature of the mental sickness.

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DISEASED UTERINE APPENDAGES AS FACTORS IN MUSCULAR AND JOINT AFFECTIONS.

Read in the Section of Obstetrics and Diseases of Women, at the Forty-third Annual meeting of the American Medical Association, held at Detroit, June, 1892.

BY M. B. WARD, A.M., M.D.,

Professor of Gynecology Kansas Medical College, Fellow of the American Association of Obstetricians and Gynecologists, President of the Western Association of Obstetricians and Gynecologists, etc.

Serious neurotic affections are so commonly attributable to diseased uterine adnexa, it is quite natural and, indeed, altogether the "latest style" to make diseased pelvic organs responsible for all morbific conditions when the true etiology is in the least obscure.

There is, perhaps, too much stress placed on the uterus and attached organs as the cause of systemic disturbances, and yet, it is frequently demonstrable that disease of these organs is the prime factor, or active agent in causing functional derangements oftentimes quite remote from the pelvis. In order that specialists shall not become too narrow in their views, it is prudent to endeavor to find the cause of nervous affections in every other part of the human economy, before any thought of the pelvic organs is considered. That is, should the patient give a clear history of suffering from disease of the pelvic organs, we should inspect with care every probable causative factor, before deciding that the entire fault is connected with the uterus or its attachments.

All are now agreed on one point, namely: that certain diseased conditions of the uterus and appendages will cause neuroses so grave in character that life is often endangered. This fact being established, general practitioners as well as specialists, should be considered extremely careless if they overlook, or fail to familiarize themselves with the histories of disturbances due to diseases of these organs.

Muscular contractions, and apparent joint affections due to para-uterine disease, especially adhesions which bind the ovaries and tubes against the pelvic fascia, are, perhaps, more common than we are accustomed to recognize. So far as my study of current literature has extended, I have failed to find anything definite regarding this factor of nervous phenomena.

Perhaps there is no special occasion to invite discussion of this phase of the subject, for it may have been mistaken diagnoses in the cases where muscular contraction of a permanent character, and apparent joint affections, have been recognized by the writer as due to pelvic disease.

The histories of the two cases which have been observed in my practice will, no doubt, enable the members of the Section on gynecology to better understand the claims of the author, than would dogmatic statements.

Mrs. F., aged 28 years, mother of two children; bed ridden for nearly one year; reduced to skin and bones; pulse 120 to 140.

Examination revealed a fixed uterus, and pelvis filled with a mass which suggested the condition of congealed wax.

The patient suffered greatly from examination. The left leg was flexed, and immovably fixed. The left hip joint was very tender to the touch and the patient presented every symptom of hip-joint disease. After making my examination, I was certain that the joint was free from disease. An operation to remove the diseased appendages and break up the adhesions was recommended only after stating that the prospects of recovery were doubtful and the patient might succumb before an operation could be completed. There was no thought on my part that the muscular and joint complication would be especially benefited by any operation in the pelvis.

The ovaries were buried in the pelvic fascia, and in removing them there was much hæmorrhage.

The inflammation had been so extensive that the ovaries, tubes and broad ligament were matted together as if they had been subjected to a white heat years before, which left them without definite form. The uterine attachment of the right tube was separated in the scooping process, and therefore, no ligating was done on this side.

The patient made a very slow and unsatisfactory convalescence, but within one year after the operation she had sufficiently recovered to do light house work, and the muscular and joint troubles were entirely absent.

The limb at the time of the operation was so firmly flexed that it was quite impossible to extend it with the patient anæsthetized, and we had to operate with the knee almost in our faces.

Case second was a girl 23 years of age, who consulted me for rheumatism, and partial paralysis of the muscles of back and lower limbs. For three years she had been gradually losing the use of her limbs and back, so that for the past year, she had not been able to walk without the use of one crutch and a cane, or two canes.

She could not turn in bed without first raising her body, and turning by the aid of her elbows. She could not lift her feet from the floor. She was a constant sufferer. She could not lie on her back on account of pain, and when she would turn on either side, there would be a dragging sensation which was anything but pleasant.

She gave a history of gonorrhœal infection contracted about four years ago. There were symptoms of syphilis, but no direct history, and after I had seen her a few times concluded that she had only been affected by gonorrhœa. She had been under the care of a large number of physicians, and treated for a number of diseases, among others hip-joint disease.

The limbs would sometimes become flexed, and she could not endure the suffering caused by forcing them straight. I watched the case with much anxiety for a few weeks before I could satisfy myself as to the cause of her deplorable condition. I examined her and could not find any masses in the pelvis, but there were strong adhesions of the uterus. She was tender but did not suffer greatly from the examination.

After I had examined her the third time I was almost

certain that there were diseased and adherent ovaries causing all her trouble, and recommended an operation. The girl cried with joy when I informed her that an operation gave promise of great relief and perhaps permanent cure.

She wished it done immediately. I operated Oct. 31, 1891. The case was thoroughly explained before the operation, and Drs. Rogers and Sheldon examined the condition of the appendages after the incision was made. The left ovary was buried in the pelvic fascia so that it could hardly be felt, and in removing it the under surface was riddled. The tube was still patulous, and not adherent to ovary.

The entire trouble seemed to be confined to the left ovary. I removed the left appendage close to the uterus. The right side was slightly adherent, but not diseased, and after breaking up the adhesions it was allowed to remain. The patient experienced no pain, and was anxious to get up in twenty-four hours.

In one week after the operation she could turn in bed as quickly as any one, and in less than four weeks she went home, walking out of the hospital without the use of a crutch or cane.

In fact she never saw either crutch or cane after she got to the hospital. The patient works around the house from morning till night, and has been on the streets making calls.

She is as nearly a transformed person as one could wish. This young lady was married the first day of this month (June).

I know of only a few cases on record where the large joints were rendered almost useless, with partial paralysis of the muscles of the back and limbs, the result of ovarian disease and impingement. One of the latest writers on pathology of ovarian disease merely mentions the fact without giving any explanation.

In February last I did an operation on a lady 35 years of age for the removal of the diseased appendages, who had serious rheumatic trouble and an apparent shortening of one limb, with tender hip and knee joints on that side, and improvement took place at once, and has continued, but sufficient time has not elapsed to report the case as cured.

If this brief paper should provoke thorough discussion of this subject or feature of pelvic complications, the writer will be entirely satisfied.

THE INFLUENCE OF DELAYED AND INCOMPLETE OPERATIONS UPON RESULTS IN PELVIC SURGERY.

Read in the Section of Obstetrics and Diseases of Women, at the Forty-third Annual meeting of the American Medical Association, held at Detroit, June, 1892.

BY L. S. McMURTRY, M.D.,
OF LOUISVILLE, KY.

The development of pelvic surgery is of such recent period that it has been necessary to revise and alter year by year and month by month many of its principles, and to constantly modify and improve its practical methods. A few years back there was but one operation practically known to pelvic surgery, viz.: ovariotomy. In a brief period it has grown and culminated in a brilliant and important branch of the healing art. This has been accomplished by the earnest and devoted labors of a few men, who have extended its scope, perfected its methods, and placed it upon the firm basis of scientific accuracy and practical demonstration. That this great work has been accomplished by a few persistent laborers is well known; that from the very nature of things it must remain a distinct specialty is incontrovertible.

It is doubtful if any special branch of surgical practice was ever subjected to such a severe ordeal of criticism as has been the new surgery of the pelvic organs. The older ovariotomists met with fierce denunciations constantly, and ofttimes were persecuted by their professional colleagues. The new surgery has been criticised with serious severity, and has been made the target for ridicule as well. Now, fortunately, its days of probation are completed, and it is firmly fixed in its position as perhaps the most brilliant and efficient department of surgical practice. The history of pelvic surgery during the past few years is in many respects anomalous. Attracted by the brilliant results of the few, it was eagerly seized upon by many untutored in the pathology of the pelvic organs, untrained in diagnostic resources, and unfamiliar with operative methods. That the aggregate of results should be disastrous under such circumstances is not strange. The anomalous feature of the situation is in the fact that those who misapply the principles of this new branch of surgery have been the most severe critics of the few who, laying aside all other work, strove to perfect its methods, and elevate it to a standard of safety and accuracy. In the evolution of no other special branch of surgical practice has such a rigid standard been established as the statistical method to which the results of pelvic surgery have been subjected. It may be fairly claimed now that the test has been made, and the decision rendered by the discriminating judgment of the profession. To maintain the present proud position of pelvic surgery and to elevate its standard of efficiency, it is absolutely necessary that certain advantages be utilized in the light of advancing knowledge and established facts, to perfect methods and thereby improve results. It is in view of the circumstances I have recited that this paper is undertaken, to show how the results of established operations for pelvic diseases may be disastrously affected by delayed and incomplete operations.

Inflammatory diseases of the uterine appendages compose the largest class of affections of the pelvic organs requiring surgical interference. In the early stages of the inflammatory process, palliative treatment, consisting in rest, depletion with saline purgatives, and local use of moist heat, may beget cure by resolution. When such a favorable termination is unattainable, when recurring attacks of peritonitis have begotten fixation and retention with irreparable lesions of ovaries and tubes, operation by abdominal section is the only rational means of restoration. Before the inflammatory mass has broken down in suppuration; before perforation of bowel or bladder, or rupture into the general peritoneum, have obtained, enucleation and removal are comparatively easy and safe. When treatment by palliative methods even after pus has formed is persisted in, or temporary safety is secured by puncture through the vaginal vault, every day of delay complicates and increases the difficulties of the operation for a radical cure, and also adds to the dangers of the operation. It is moreover an established pathological fact, supported by abundant clinical testimony, that secondary lesions of the kidneys and lungs often follow from prolonged suppurative changes in the uterine appendages. The depuration of the blood often hinders convalescence after successful operation, and the opium habit so frequently associated with pelvic inflammations may remain as a miserable legacy to the long-suffering patient. Intestinal complications are the most common and serious difficulties encountered in operations for inflammatory diseases of the appendages. Unless the operator is prepared to deal skilfully with a torn intestine or wounded bladder, he assumes unwarranted

responsibility in undertaking neglected cases, for he will frequently determine the fate of patients by the manner in which he repairs these lesions. All this could be averted, dangers and complications reduced to a minimum, the patients spared prolonged suffering, and the results of treatment made complete with increased safety, by prompt resort to operation. Moreover I will endeavor to show, in the course of this paper, that delayed operations upon the inflamed appendages are very frequently incomplete, and hence are disastrous, either ending fatally at once or after prolongation of the disease.

Fibroid tumors of the uterus, or more properly, myomata of the uterus, constitute another form of intra-pelvic disease in which the disastrous results of delay are conspicuous. These tumors were formerly considered very innocent, and by many practitioners they are now treated as of trivial import. The soft myoma, prone to cystic degeneration, œdema and suppuration, is one of the most formidable and deadly conditions with which the pelvic surgeon has to deal. These tumors are almost invariably associated with disease of the Fallopian tubes and ovaries. In my latest experience with an uterine myoma, three weeks since, the rupture of the Fallopian tube on the right side flooded the entire pelvis with pus. This is not an unusual complication. Fibroid tumors produce persistent uterine hæmorrhage by which the patient is exhausted as with profound anæmia. When submitted to operation otherwise safe, these cases are often lost from the extreme anæmia which has obtained. In the early stage of these growths, before the appendages are carried behind the tumors or incorporated in the mass, the removal of the uterine appendages is a safe and efficient operation. The mortality of this operation in skilled hands is very small. It arrests hæmorrhage and, as a rule, arrests the growth of the tumor. In my own work this operation has been among the most satisfactory in its immediate and remote results. It is in connection with a consideration of this operation that Mr. Lawson Tait exclaims: "The whole of my experience in every department of abdominal surgery is one continuous outcry against delay."

Since we have learned more of the complications and possibilities of fibroid tumors of the uterus, it is known that in a considerable proportion of cases the tumor not only does not cease to grow at the menopause, but in fact grows more rapidly after that period. As Skene states, "the organic forces which maintained the menstrual function being no longer called for, are devoted to the growth of the myoma." I have done abdominal hysterectomy in one instance in which the tumor took on its most active growth after the menopause, and when the patient was in her fiftieth year the tumor had risen to the diaphragm almost. With profound anæmia, interference by pressure with the digestive functions and circulation, irritation of bladder and rectum, the patient is ill prepared to withstand any serious operative procedure. In recognition of these facts, Price, whose experience with these tumors is the largest and whose results are the best, has made a plea for early hysterectomy as did Bantock for early ovariotomy. When his plea shall have been adopted, we will witness the same improved results as obtained in response to Bantock's plea for early ovariotomy.

It would seem that the necessity for prompt resort to operation in cases of ruptured tubal pregnancy would need no argument or emphasis, in view of the deadly nature of the lesions involved. Yet we hear from time to time of a conservative method which would await the progress of symptoms and preparation of the patient and environment for operation; and the number of deaths annually from ruptured ectopic pregnancy without operation is large. Oftentimes the character of the lesion is unrecognized, but quite often death results from delayed operation. Surely no surgeon would advocate delay in dealing with a divided femoral artery. The same positive indications exist for immediate operation in cases of ruptured tubal pregnancy. The surgical rule to cut down upon and tie the bleeding vessel without a moment's delay is as applicable in one case as the other.

In studying the results of treatment of intra-pelvic disease, it will be apparent that next to delay in operating, incomplete operations are the most disastrous. It is evident that the cumulative difficulties encountered by the operator are most frequently the results of prolonged delay, as has been already indicated.

The exploratory incision is a diagnostic resource of inestimable value. It has a great field of usefulness, and the knowledge acquired by its aid, converting uncertainty into certainty, opinions into facts, is a guide to action not possible by any other means. It is apparent, however, that the limitations of the exploratory incision will be narrowed in proportion as skill in operating improves. To a certain extent almost all operations for intra-pelvic disease are in their inception exploratory. But it must be conceded that many unfinished operations are classed as exploratory. It should be our aim to carry the operation to completion in every possible case. There is an ever-present temptation to avoid the immediate dangers of a severe, difficult and prolonged operation, at the expense of the deferred but assured result of a mortal disease.

In dealing with large tumors, ovarian and uterine, in operations upon the appendages for long-standing inflammatory lesions, the greatest difficulties arise from adhesions. These difficulties will be greatest in neglected cases of suppurative disease of the tubes and ovaries. Here all landmarks are lost, the pelvic tissues are agglutinated and fused into an undistinguishable mass. Injury to viscera and hæmorrhage are ever-present and often inevitable dangers. These are the operations most frequently left unfinished, and if the patient recovers from the operation, it is to succumb at last to the disease. It is remarkable how long some women can live and suffer with persistent disease of this character. In one case coming under my observation the disease had been in progress fourteen years. Discharge of pus had taken place repeatedly through rectum and bladder, with only temporary relief. The entire pelvic contents were matted together, making enucleation difficult in the extreme and rendering the result imperfect. Prompt resort to operative treatment would have prevented incalculable suffering and restored the patient to perfect health.

The influence of improved diagnostic and operative skill upon the results of pelvic surgery must be apparent to every one who has watched the progress of this branch of surgery. Formerly it was quite common to observe in the reports of work by representative special surgeons that thirty or forty ligatures were applied to bleeding points; that adhesions were

divided between ligatures; and that tumors and broken down tissues were left as irremovable; or that the growth was not disturbed, as it was most probably malignant, etc. With revised methods and improved skill such practice has been replaced by careful, thorough and bold enucleation. Now, by means of forci-pressure, hot water and gauze packing, hæmorrhage from severed adhesions is controlled without exposing the patient to the shock of prolonged operation under anæsthesia. With these improved methods the number of incomplete operations has diminished, and as individual skill improves and operative methods are perfected, the proportion will be still further lessened.

There is another influence which is destined to be very potent in improving results now impaired by delayed and incomplete operations. As knowledge is diffused and diagnostic skill improved, the family physician will recognize serious pelvic disease at an earlier period, and advise operation. With improved results, both the profession and the public will cease to regard peritoneal operations with the fear of the olden and mortal era, and thus delayed operations will be lessened. This should be the aim of every conscientious worker, and it is the purpose of this paper to show how essential are *promptness* and *thoroughness* in applying the advanced principles of modern pelvic surgery.

Discussion.

Dr. G. Betton Massey, Philadelphia, thought it unfortunate that these two papers were read together, as their conclusions are very different. The paper of Dr. Ashton was the result of careful work, as I well know. The paper of Dr. McMurtry seemed to be a collection of dogmatic statements based solely on his own opinion. He urged a complete operation, and said that we could not always say a woman died on account of the incompleteness of the operation. It is a little behind the times to advise the practitioner to send all his cases to the abdominal surgeon every time they have a pain in the belly. I venture the prediction that in the future serious interference with the abdominal cavity will be limited to those cases in which pus has been demonstrated to exist in the pelvis. He wished to call attention to the ease with which a powerful electric light could be enclosed in a series of test tubes, one inside of the other according to the amount of heat. This can be inserted into the vagina and will light up the pelvic cavity. This light will penetrate a cyst, but will not penetrate a collection of pus, which is a means of diagnosis of the presence of pus. This light will penetrate living bone even better. We can notice light shining through our fingers on holding them up before a bright light.

Dr. R. B. Hall, Cincinnati, thought it impossible to determine the presence of pus in all cases, and that the prediction of Dr. Massey would not hold, and that this would be dangerous doctrine to promulgate. The doctor reported a case in point.

Dr. Joseph Tabor Johnson, Washington, D. C., thought the papers read, and others which have been recently published, teach us that we are not thorough enough in our operating.

Dr. Franklin H. Martin, Chicago, thought the paper of Dr. Ashton taught us how to avoid trouble. We should give much care to the treatment of raw surfaces in the intestine. These surfaces will readily take on adhesions. These should be covered with peritoneum if possible, otherwise covered with an omental graft. Stumps should be covered with peritoneum, which will also prevent attachments. Saline treatment is very important. In the Woman's Hospital at Chicago we endeavor to get a passage of fæces or gases during the first twelve hours.

Dr. Edwin Ricketts, Cincinnati, has had much benefit from the administration of strychnia in large doses in intestinal paresis. A case which he recently had looked very much like peritonitis, but after the administration of strychnia a large quantity of gas was discharged. The speaker could not agree with Dr. Martin as to the danger of adhesions resulting from the stump. Such had not been his experience.

Dr. W. E. B. Davis, Birmingham, Ala., did not think abrasions had such an influence on the causation of adhesions as claimed. Purgation twenty-four or thirty-six hours after operation will not break up adhesions. It should be done earlier. He considered it very important that the bowels be returned within the abdomen during the operation. In fibroids it is the adhesions which give symptoms, and it is unjust to blame these on the electrician.

Dr. E. P. Davis, Philadelphia, said the pathology of the peritoneum was a very important feature. Different portions of peritoneum have different powers of resistance. Iodoform seems to be least irritating to the peritoneum, next boracic acid. It is known that the peritoneum of dogs is different from that of the human being.

Dr. W. E. Ashton, Philadelphia: Obstructions are not always due to faulty technique; sometimes to inflammation which could not be prevented. Taking a graft from the omentum only made a point where the gut might pass through. Strychnia has been followed with very good results. I give one-fifteenth to one-tenth grain three or four days before and after section. It increases peristaltic action of bowels, clears them of fæces and lessens the danger of intestinal paresis.

Dr. L. S. McMurtry, Louisville, in reply to criticism, said that the observations and deductions made in his paper were based upon his personal experience in pelvic surgery, and were fortified by illustrative cases. He deprecated resort to operation in doubtful cases, and advocated most careful discrimination in deciding the question of operation in all cases; but he believed it equally an error to delay operation in clearly defined conditions requiring operation until the patient's chances of recovery are reduced or lost. Of course, there must be a restriction on early operation in fibroid tumors in accordance with the rapidity of growth and amount of hæmorrhage. When these conditions clearly point to the necessity for operative interference, the sooner it is done the better. When resorted to early, the appendages may be removed and the growth, as a rule, will be arrested and hæmorrhage cease. If hysterectomy is rendered necessary, the sooner it is done the safer it is for the patient. It has been demonstrated that electricity is a failure in the treatment of uterine myomata; and while these tumors sometimes disappear at the menopause, or sooner, without treatment, they often grow in spite of the menopause and treatment, and unless removed destroy the patient's life.

COLPO-PERINEORRHAPHY.

Read in the Section of Obstetrics and Diseases of Women, at the forty-third Annual Meeting of the American Medical Association, held at Detroit, June, 1892.

BY EDWARD W. JENKS, M.D.,

OF DETROIT.

INTRODUCTION.

In 1877, I read a paper before the Michigan State Medical Society in which were mentioned "some new procedures in the operation for laceration of the perineum" and Jan. 8, 1879, I read a paper before the Cincinnati Obstetrical Society, which was published in the *American Journal of Obstetrics*, for April, 1879, describing the same procedure as in the first mentioned article, the latter being entitled "Perineorrhaphy and a Description of a New Mode of Operating." This paper advocated a new mode of submucous dissection of the tissues or what is now designated flap-splitting.

This mode of operating was favorably commented upon by the leading medical journals of the day and the leading text books on gynecology published in this country continued for many years to sanction it. In that paper the cutting away of the flap instead of preserving it was advised. Subsequently I preserved the flap, and believing for a time my mode of operating as well as the preservation of the flap was original, so stated when asked regarding it by some of my friends, notably the president of this Association (Dr. Marcy), who mentioned the matter in one of his publications. But learning that others had also described saving the flap no claim has been made as to

its originality with myself. The submucous dissection I described, or flap-splitting operation as now designated, was made several hundreds of times by me in public clinics, as well as by many others who had adopted it, some years before the flap-splitting operation of Tait or others had become known.

However, I am not strenuous in claiming priority in the mode of operating I described so long ago, as it is of little importance, for I am only one of the great multitude working in the field of gynecological surgery.

I am prompted to this historical fact as an introduction to this paper as the surgical procedures I made known fifteen years ago are occasionally referred to at the present time; and a late edition of a well known text book that had previously commended it by means of lengthy quotations and illustrations now dismisses it by a brief allusion as "too bloody."

Although repeatedly asked to write an additional paper I have had other fields to cultivate, and what seemed more important in surgical matters to engross my time and attention, but of late there seems to have been a revival of interest in plastic gynecological surgery.

DESCRIPTION.

The operation I make to-day and which is described in this brief paper is, I believe, a great improvement on my former method. It would hardly be possible for one to make hundreds of operations of any kind without finding a chance for some improvement either in the mechanical portion or in results obtained.

Of the surgical devices before the medical public known under the general title of perineorrhaphy the name is legion, consequently the merits or demerits of other operations than the one here described will not be considered.

It is not my purpose to discuss laceration of the perineum or the necessity for repair in detail. It is well known to every careful observer that rather extensive laceration of the perineum and vaginal walls occur in childbirth from which no bad results follow. Again it is equally well known that many discomforts and divers reflex symptoms often ensue from lacerations that at the time of their occurrence seem too insignificant to demand a passing thought or even a single suture.

It is to the consideration of secondary operations alone that your attention is called. I will state as a general proposition that operations are not demanded because of laceration *per se*, but when there are unmistakable discomforts that can be plainly traced to them, and health and comfort can only be recovered by restoring the torn parts to their normal relations.

The portion of the recto-vaginal septum known as the perineum supports the lower portion of the posterior vaginal wall, which in turn supports a corresponding part of the anterior vaginal wall. The lower portion of the rectum is sustained and the proper performance of its functions aided by the perineum. Four muscles, the levator ani, sphincter ani, transversus perinei and bulbo-cavernosus are here united; and it is the severance of these from their fellows on the opposite side together with the separation of the perineal fascia which produces the mischief, therefore laceration of the perineum and a portion of the posterior vaginal wall, whether partial or complete, may cause a variety of conditions, such as loss of vulvar integrity and impairment of the functions of the rectum, partial or complete incontinence of the rectum and bladder, increased and irritating secretions of the vagina and rectum and recurring prolapse of the rectum after operation for prolapsus recti, descent of the recto-vaginal septum or rectocele, a similar condition of the anterior vaginal wall and bladder or cystocele, and descent of the rectum. There are also various neurotic and sympathetic disorders which it is needless at this time to dwell upon.

I have seen quite a number of patients that had been operated upon with the result of having, to all external appearance, a perfect perineum but with a rectocele above the line of the dissection or flap-splitting that formed an excellent pocket for the accumulation of uterine and vaginal secretions, in which the neck of the prolapsed uterus was constantly macerating. In many of the cases where only a portion of the redundancy is remedied the only benefit seems to be, to provide a better support for some form of pessary to sustain the uterus and vaginal walls.

My own observation and experience, which I presume agrees with others, is that there are, at least in a general sense, four important ends to be attained in repairing the class of injuries under consideration: 1. To restore the loss of power and function to the lower portion of the rectum and vagina. 2. To restore the normal sustaining quality of the posterior vaginal wall for the anterior vaginal wall and bladder. 3. To provide as much support for the uterus as the perineum naturally gives. 4. To cure the many distressing nervous accompaniments.

Any surgical procedure which does not obtain such results to a great degree is not in a strict sense successful.

A perineum may be operated upon and, as far as external appearances are concerned, is successful, but if above the point of dissection there still remains a redundancy of the vaginal walls, or the restoration is not sufficient to support the anterior vaginal wall the operation is but partially successful. This is true whether the uterus is held up to the health line or not. There are so many causes operating to produce uterine displacement, that it is not as a general rule just to guage the success of an operation by the measure of uterine support it secures. For the reason that these partial operations are often insufficient and are not followed by the anticipated beneficial results, I have chosen to designate the surgical procedure I have been making for a number of years, colpo-perineorrhaphy. To accomplish the best permanent results it is essential that dissection of the flap extend as high within the recto-vaginal septum as there are signs of slack or redundancy of the posterior vaginal wall.

My mode of procedure is as follows: I first nick with the scissors each labium to mark either termination of the anterior margin of the flap and then, having introduced two fingers into the rectum and assistants making the parts taut, I insert the sharp pointed scissors near the juncture of the integument and mucous membrane in the median line or sometimes on one of the nicked lips and proceed to dissect a flap up the septum as far as redundancy of the walls can be observed. (Fig. 3.) It is important for the sake of making a more rapid and neat operation that the dissection be made in its entirety without withdraw-

ing the scissors. When I first described my mode of operating in 1877, my friend, the late Dr. Albert H. Smith, devised a knife to take the place of scissors and subsequently I caused a knife to be made larger and more flexible than his. (See Fig. 1.)

The scissors which I have used for the past five years are slightly curved with blades completely overlapping each other, and both inner and outer edges ground equally sharp. I have found this form

Figure 3.

of blade is less liable to become entangled in submucous dissections than the ordinary shaped blade. (See Fig. 2.) These scissors can be used alternately in their proper capacity or with blades closed as a knife to dissect the flap more perfectly, as their sharpened outer edges and points render this an easy task, and there is a great advantage in their use in cases where there is much bleeding, as there is less hæmorrhage than with an ordinary knife.

ent directions; still I do not claim any originality in this matter and wish the credit to be given to whom it is due.

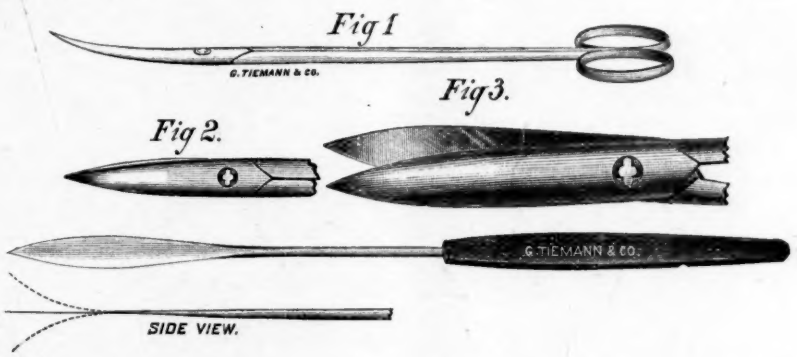
No better results have ever been obtained than by silver sutures, but on account of their stiffness they cause more pain, therefore, in extremely sensitive patients I substitute the silk-worm gut, which possesses the principal advantages of silver wire and is not so unyielding. Kangaroo tendon is also a safe

Figure 4.

and useful suture, as its time of continuity is from fifteen to twenty days.

The needles are a straight, flat non-cutting needle about two inches in length and a slightly curved Peaslee needle.

The latter is provided with a carrying thread which is number 4 or 5 braided silk about eighteen inches long. The former is used only for the short and superficial sutures which are threaded directly into the needle.



One objection recently made to this operation is that it is bloody and sometimes one or more arteries have to be tied.

To my mind, if that is the case, the surgeon of the present day is not intimidated by a few small spouting vessels and if such a calamity occurs it is quickly and easily remedied.

In many hundreds of operations I have not been compelled to ligate vessels, all told, more than six times.

The next important step in the operation after the dissection of the flap is the insertion and adjustment of the sutures. I was led many years ago to adopt the method of the late Dr. Agnew of Philadelphia, in placing sutures in the perineum. Since then I have carried them deeper, higher and in somewhat differ-

In the majority of methods of operating for incomplete laceration, the first and frequently the second sutures are shorter and of far less importance than the third or fourth, or fourth and fifth, as the case may be. But in my operation matters are reversed, for the first two sutures are the longest and most important. Indeed, for want of a better term I often call them *the parent stitches*. (Fig. 6.)

The first assistant lifts up the flap by means of a tenaculum hooked into the edge at the center. (Fig. 4.) Introducing two fingers of the left hand into the rectum to guard against wounding it, I start the needle in at the distance of one-third to one-half inch back from the denuded surface, and turning the point well toward the left buttock and the handle correspondingly as far in the direction of right buttock, I

push it rather deeply into the tissue of the anterior ischio-rectal space, then upwards and finally inwards along recto-vaginal wall until it has been carried just above the highest point of dissection in the center, at which location or as near to it as possible the needle-point is brought out. When the point is about to come through, if counter-pressure is made with a blunt hook, the needle can be pushed through more easily, with less strain upon the septum and with less likelihood of pricking the anterior wall by the

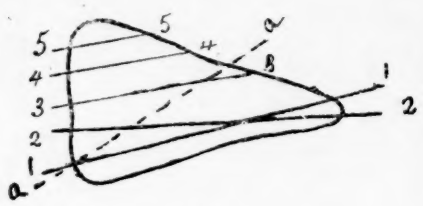


Figure 5.

sudden emergence of the needle than without its use. As soon as the eye appears a loop of the carrying thread is pulled out by a tenaculum and the suture passed through it. When the needle is withdrawn one half of the first stitch will be *in situ*.

The needle in then introduced in the same manner in the opposite side, the upper end of the suture threaded into the loop and the other half of the stitch carried to place.

For the second stitch the needle is started in about a third of an inch above the first and its point directed at first outward in the same manner as in the introduction of the first suture. Not quite so much lateral tissue is taken up this time, that is, the needle does not make quite so wide a side sweep for the second suture but passes more directly up along the recto-vaginal septum and when it has reached the upper third of its course, *crosses* the first suture and comes out on the vaginal mucous membrane about one-half or two-thirds of an inch above the central highest point of dissection. After drawing the first half of the suture into place the needle is introduced in the same manner on the opposite side for the second half of this stitch.

For the third stitch, a third of an inch above the second, the needle passes along the denuded surface till it reaches the line of junction of the septum and the flap, when it enters the latter at about its upper fourth, burrows across to the opposite side and down the denuded surface to the outside.

This stitch can sometimes be introduced in one continuous circuit, in other cases one-half at a time.

The fourth and fifth stitches are buried under the denuded surface as far up as the junction of the septum and flap where they pass under the flap without burrowing in it to the opposite side. With each of these sutures it is usually more convenient, although not necessary, to put in one-half and then introduce the needle on the opposite side in the same manner for the other half.

The ends of the sutures are now placed together and traction enough made upon them to determine whether the denuded parts are coming properly into apposition. At this time the flap will emerge more or less from the introitus and will frequently have the appearance of being much too long; the operator will consequently be tempted to pare off a quarter of an inch or more from the anterior edge, but this should not be done except in rare cases of great redundancy. This slack is disposed of by the gradual retraction of the flap during the process of healing and settling into normal relations. The sutures are now loosened again and the sixth stitch introduced which is designed to purse up the anterior side of the flap and also bring together the last of the denuded surface. For this purpose the straight thin needle previously mentioned is used. The needle is passed under the portion of denuded surface contig-

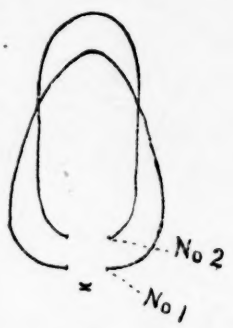


Figure 6.

uous to the edge of the flap and thence into and through the latter to the opposite side. All the sutures are now picked up and slackened enough to allow the flap to be raised in order to clear out by the douche any clots that may have collected.

The sutures are then all drawn up ready for fastening.

If silver wire has been used each suture is carefully shouldered and twisted. If silkworm gut has been employed the sutures may be either tied or secured by perforated shot.

Care is required in adjusting the first two long sutures not to draw them too tightly, otherwise they will cut in a little, and owing to their including so much tissue adjacent to the rectum, will be painful. The third, fourth and fifth sutures can be drawn more tightly. The sixth, again, should be but moderately tight, as the pursed up edge of the flap will not bear too much constriction.

Usually two or three superficial sutures of fine silk, horse hair or small silkworm gut will be required to coapt any raw edges turned out by the puckering up of the flap.

In case there seems to be any liability of hæmorrhage beneath the flap I place a strong silk suture by means of a Peaslee needle outside of the adjusted sutures and over the flap, which I retain for about twenty-four hours and then remove. (Fig. 5, *a*.)

These last are not absolutely essential but they give a neat appearance to the operation as well as dispose of surfaces for absorption or granulation. If silver wire has been used the ends may be massed together and inserted into half an inch of small rubber tubing to prevent them from pricking the patient.

In any surgical work, but especially in the plastic operations of gynecology, it is extremely convenient and helpful to an operator if he is ambidextrous. However, but few possess this accomplishment to a full degree and it is not really a *sine qui non* to skilful operating. Many expert operators do not interchange much the work of the two hands in the use of the knife, scissors and needle.

In this operation or almost any other for perineorrhaphy, the long stitch or stitches which pass from

the outside up to the highest point of dissection in the vagina should always be put in one half at a time. If the dissection is made even approximately as high as it should be, a slightly curved needle cannot make the whole circuit at once, except in a patient with lax tissues and a broad space between the tubera ischiorum.

But even when it can be done it is accomplished at the expense of considerable strain upon the parts operated upon and the whole vaginal column.

I have constantly mentioned six as the number of sutures employed, but only because that is the number most commonly required. Occasionally five are sufficient, and sometimes seven or eight are needed.

As there are no exposed raw surfaces either externally or internally, but little dressing of any kind, and but few vaginal douches are demanded. Three or four are the usual number; one at the end of thirty-six hours, another on the fourth or fifth day, and another on the morning of the day the stitches are removed, usually the seventh. The external parts on the other hand, require about the same attention as in other perineal operations. Night and morning, and each time after urinating the soft parts adjacent to the line of union, and also the buttocks are carefully separated and the wound and the surrounding parts gently irrigated with sterilized water or a one to six thousand solution of bichloride.

The surgical procedure which I have here described under the name of colpo-perineorrhaphy, cannot commend itself on account of the consummate ease or rapidity in which it can be made. It is not as easily or quickly done as ordinary perineorrhaphy, nor even as the flap-splitting operation of which so much has been written of late.

But after essaying different operations from Baker Brown's to many of the present day, I have settled upon the method I have here briefly outlined as the best one I can make for the great majority of cases that present themselves to me for treatment.

In conclusion there are a few points to which I wish to direct attention.

1. Any single mode of operating is not adapted to every case of laceration of the perineum.

2. All other qualifications being equal that surgeon will be the most successful in this class of operations, who, instead of following hard and fast rules, possesses a mechanical skill which he can adapt to the peculiarities of each individual case.

3. The subsequent comfort of patients is not facilitated by superabundance of cicatrical tissue within the vagina; therefore the anterior wall, instead of being subjected to any surgical procedure for redundancy should be sustained by a restoration of the normal posterior wall.

4. The surgical operation here advocated has for its object, a restoration of the torn posterior vaginal wall and perineum to its normal condition, whereby there is afforded (*a*) support for the uterus to the full extent provided for in the vaginal walls; (*b*) support for the anterior vaginal wall and bladder; (*c*) support for the lower end of the rectum.

84 Lafayette Avenue.

THE American Microscopical Association will meet in Rochester, N. Y., August 9, and not August 10, as heretofore announced.

OVARIOTOMY IN THE PRESENCE OF PREGNANCY.

Read before the Section of Gynecology and Obstetrics, at the Forty-third Annual Meeting of the American Medical Association, at Detroit, Mich., June, 1892.

BY WILLIAM H. MYERS, M.D.,
OF FORT WAYNE, IND.

Seventy-six (76) years have elapsed since Recamier presented to the medical world the uterine speculum, and the dark age of uterine pathology was passed and a new era begun.

As in history, we progress by alternation, by conflict and by revolution, so were these the steps in medicine and surgery by which we have attained the advanced thought of to-day.

From 1816 to the present, a wide range of material has been submitted; brilliant at the time, but existing now as the residue, as merely a trace of a previous pathology.

Recamier taught that the primary element of uterine disease was inflammation and ulceration; Bennet, ulceration of the cervix; Tyler Smith, hyper-secretion of the mucus glands of the cervix; Meigs, retro-version; Velpeau, flexion; Lisfranc, congestion and engorgement of the cervix; Madame Bovine, uterine catarrh; Grailly Hewitt believes that all the ills of woman are due to uterine displacement; Tilt, ovarian peritonitis. I might in this connection refer to the labors of Goupil and Bernitz in pelvia peritonitis; for they were the first to teach us a correct pathology of this condition, and the secondary importance of cellulitis. We of the present accept with the greatest facility the doctrine that the peritoneum and the ovaries are the structures underlying nearly all of the phenomena presented in uterine disease, and we relegate cellulitis. The truth of this doctrine opened the field of intra-peritoneal operations, and ovariotomy, according to a recent writer, passed through three successive periods, a period of groping, ending with the successful work of W. L. Atlee, Baker Brown and Sir Spencer Wells; second, a short period of excessive specialization; and third, a period of popular adoption deriving its impulse from the introduction of antiseptics.

The treatment of ovarian cysts has been settled, but the presence of an ovarian cyst during pregnancy presents questions for our consideration. The doctrine advanced may be thus enumerated:

1. Non-interference.
2. Puncture of the cyst.
3. Induction of premature labor.
4. Porro's operation.
5. Ovariotomy.

In regard to the question of non-interference, in the light of modern surgery, we dare not entertain it, for the dangers of axial rotation of the tumor, rupture of the cyst, of impeded labor, rupture of the vagina during labor, far outweigh the dangers of the removal of an ovarian tumor. The exceptional cases reported, where pregnancy has coexisted with an ovarian tumor, and the woman has passed safely through confinement, are too few to be accepted as a precedent; for in a large proportion of cases, if the tumor be large, abortion will occur; if not, a lingering labor and still-born child will be the result, and the danger of sudden death be ever impending. In a paper by Doctor Playfair, published in the ninth volume of the Obstetrical Transactions, he reports

thirteen (13) cases left to nature; seven (7) recovered and six (6) died. Spencer Wells enumerates three (3) cases where death followed the spontaneous rupture of an ovarian cyst in the sixth or seventh month. Doctor Barnes has seen one (1) case where death followed rupture of the cyst. In another case, the patient at last had peritonitis and died. Rokitausky refers to similar cases. I believe the almost unanimous opinion of the medical profession to be, that it is not safe to allow an ovarian tumor to progress without interference in the presence of pregnancy.

Puncture of the cyst, I believe, ought only to be resorted to where the cyst is simple and an operation for its removal would be impossible, as when called to the patient during labor. An ovarian cyst is more dangerous during pregnancy than at any other time, and that it will relieve the distension with a very slight risk to the mother, unless ovarian fluid should escape into the peritoneal cavity, or air enter into the peritoneal and into the cavity of the cyst. He also informs us that the cyst must be single, and if multilocular, tapping can be of little use.

I will here admit that there is no steadier brain in the medical profession than Sir Spencer Wells', equally at home in generalizations and in details. It is not my purpose to enter into any detailed criticism of his opinions, but we will refer to conflicting authority in subverting the view that "tapping will bring only a slight risk to the mother." In 1869 Thomas Nunnelly, of Leeds, England, delivered an address in which he stated, "in our infirmary of the last thirty-eight (38) cases of paracentesis for ovarian disease, ten (10) ended fatally." Stilling tells us that no surgeon should ever puncture an ovarian cyst. Storer says, "Never tap an ovarian cyst. It is a crime." If I were asked the question, "why not?" my reply would be, "after its adhesions form, it is useless and never cures." Charles Clay of Manchester, considers it useless, dangerous and to be followed only by evil. In a case of ovarian tumor, unilocular, that came under my own observation, tapping was resorted to in my absence and the patient died of peritonitis on the fourth day. Pozzi uses the following language: "Puncture is only to be resorted to when it is absolutely impossible to operate on the cyst." One of our latest authors, Bland Sutton, in speaking of paracentesis and of its several risks, says, "If air be admitted into the cyst during the procedure, or the instrument be septic from want of care and cleanliness, or if the fluid leak into the peritoneal cavity, and should it possess irritating qualities, inflammation and suppuration followed by fatal peritonitis may be the consequence. If these results follow the puncturing of an ovarian cyst in the absence of pregnancy, how much more likely to occur if complicated with pregnancy.

Induction of premature labor.—In 1869 this was advocated by Doctor Ruth, of London. Doctor Barnes had often met this complication by inducing premature labor, for the reason, he claimed, that nature could not tolerate the double uterus and a growing ovarian tumor. These opinions were expressed in in the Obstetrical Society of London in 1869, and at the close of the discussion, the President remarked that the sentiment was in favor of inducing premature labor. I may here state that the prevailing opinion now is that the induction of premature labor places the mother's life in peril and implies the sacrifice of the child.

Ovariotomy.—Tait states the accepted doctrine to be to perform ovariotomy and let the pregnancy alone. I believe this teaching to be good and the practice worthy of adoption. If the question were to be settled by statistics alone, I might refer to Olhausen; up to 1885 Schroeder had performed ovariotomy during pregnancy in twelve cases, Sir Spencer Wells in ten (10), Olhausen in eight (8), Tait in six (6). In only one of these thirty-six cases did the patient die. In this connection I will refer to Sir Spencer Wells' cases and the results to the mothers and the products of conception. Four (4) were operated upon at the third month; all the mothers recovered and pregnancy only interrupted in one case; three (3) were operated upon at the fourth month; mothers and children all did well; three (3) were operated upon at the sixth and seventh months; one woman died five days after the operation, one child born twenty-two days after the operation, one fœtus expelled nine hours after the operation, and one child born one day after the operation.

Where shall we interfere? There is a time in most cases when the balance of circumstances is favorable to operations, on the side of the patient. With a rapidly enlarging tumor and with pregnancy advancing it is easy to lose the golden opportunity by delay.

> "That gentle physic given in time had cured me,
> But now, I am past all comfort here, but prayers."

In 1872 Peasley wrote: "If we delay until the health is somewhat impaired, we diminish the risk of peritonitis, which destroys one-fourth of all who die after the operation. This opinion was endorsed by Atlee, Bradford, Tyler Smith and Erickson. With this doctrine Peasley imbued the whole professional mind of America. He followed this practice, had seventy-eight (78) ovariotomies with thirty-nine (39) deaths.

We emerged from this doctrine in 1884. On a former occasion while reviewing this doctrine, I said that the time has gone by when we believe that the peritoneum has become more tolerent of injury and less liable to inflammation, when "The eye doth lose its lustre, and like the world's great warrior, the woman cries, 'Give me some drink, Titinius, as a sick girl.'"

Not often can an important doctrine gain immediate acceptance, but more likely the mental evolution necessary to establish, to recast old views and lead to new practice will be of slow development. "It often takes twenty years for the old people to make up their minds to leave the old farm."

The doctrine taught by Bantock, of London, with regard to early operation is as true in the presence of pregnancy as in its absence when he asserts, "I would urge then with all the force that the strongest conviction imparts that ovariotomy should be performed as soon as we can be sure of the diagnosis." This teaching is in accord with the first principle of scientific surgery. We inculcate the doctrine of early removal of calculi in the bladder, gall-stones in the cystic duct, and renal calculi, for the reason that there is no tendency to spontaneous cure and the surgical procedure becomes a necessity. In favor of ovariotomy in the early months of gestation we have this to say: The prognosis will depend largely upon whether the case is a simple or complicated one, the presence or absence of adhesions, and the length of time occupied during the operation. That the exis-

tence of adhesions greatly interferes with the success of the operation has been proven by Bantock, by referring to one thousand cases in Mr. Wells' practice. Adhesions, 599 cases, 165 deaths, 27 per cent. No adhesions, 401 cases, 68 deaths, 16 per cent. In his own practice, in one hundred eighty-five cases: Adhesions, 125 cases, 25 deaths; no adhesions, 60 cases, 3 deaths.

The Case.—In 1883, Mrs. B., residing in Angola, Indiana, at the age of 17, was afflicted with a severe attackof scarlatina; sequelæ, ascites anasarca. Recovering from this, she was married December 24, 1889, pregnancy discovered in May, 1890. In September, 1890, she suffered intense pain in the lower part of the abdomen, depriving her of rest; she was unable to lie down at night. She was enlarging rapidly and with it her distress increased. She would not consult a physician, being a firm believer in the so-called Christian Science. She at last felt compelled to consult a physician. Dr. Waller of Angola was called, and his diagnosis was pregnancy complicated by ovarian tumor. He requested me to visit the case with him, and I saw her October 1, 1890, and verified the diagnosis of Dr. Waller. We urged upon her the necessity of an operation. She gave her consent and we operated upon her October 5, 1890. After the usual aseptic precautions, an incision in the median line three inches in length disclosed an ovarian tumor on the right side, pushed up by the enlarging uterus. It had a broad pedicle, and it was in axial rotation. The physiological development was easily defined from the pathological by the difference in color. The tumor was multi-locular, weight of cyst and fluid contents probably ten pounds. The few adhesions were easily broken up by sponge pressure after the cyst was emptied. The pedicle was then transfixed and ligated by silk thread, and the cyst removed. The abdominal cavity was carefully washed out with warm water, a drainage tube inserted, after which the wound was closed and dressed in the usual manner, and the patient placed in bed. The shock was not severe, although she was restless for a few days, without alteration of temperature. October 12, uterine pains came on and the fœtus was expelled on the 16th without much suffering. This was followed by rise of temperature 102 degrees, chilly sensations. The local douche of hot water carbolized was resorted to twice in twenty-four hours. After the twentieth day from the operation, progress was satisfactory, and she is now enjoying excellent health.

In conclusion, gentlemen, the diversity of opinion concerning the treatment can easily be explained. The obstetrician regards premature labor with the greatest favor, the gynecologist abdominal section. From the latter, we receive innumerable instructions from time to time, a profusion of counsels under the sway of theoretical ideas. Success depends only upon a question of material; the personality of the surgeon is effaced, and if he works in a costly furnished room and with very expensive pieces of dressing, he can practice modern surgery and must succeed. Now I regard this as a dangerous doctrine that effaces the surgeon behind a question of material. Antiseptic surgery is governed by questions of material. To practice it successfully demands all the science of former days, more ideas and qualities unknown to our predecessors. Possessing these, the surgeon must be able to practice everywhere, and his success will increase in a direct ratio with his experience, and that is independent of antiseptics, but mainly governed by habits of cleanliness and fertility of operative resources that come by experience. John Homan says: "in regard to hysterectomy, I performed the operation much better than I did years ago, and my last cases nearly all recovered." Thomas Keith, speaking of his supra-vaginal hysterectomies, says: "Without the experience that ovariotomy has given me, I shrink from thinking what the mortality might have been."

AMERICAN MEDICAL ASSOCIATION.

SECTION OF PRACTICE OF MEDICINE.

First Day—Tuesday, June 7.

Isaac E. Atkinson, M.D., Chairman *pro tem;* James M. French, M.D., Secretary.

The proceedings of the Section were opened with an address by Dr. R. T. Edes, Chairman of the Section, read, in the absence of Dr. Edes, by Dr. E. P. Gerry. The title of the address was:

RELATIONS OF BACTERIO-CHEMICAL RESULTS TO PROPHYLAXIS AND THERAPEUTICS.

In opening the meeting of this important section, it is appropriate, as well as in accordance with the regulations of the Association, that some general remarks should be made in the nature of a recapitulation of recent advances in our science and art; a stepping aside, for a few moments, from among the bustle and hurry of practical work with its infinity of details, its confusion and worry; a looking over the field of battle from the outside; an orientation and correction of the compasses for a fresh start; or, as our commercial friends would say, taking account of stock for another year's business.

It is manifestly impossible, in the forty minutes wisely assigned as the maximum limit of time for these remarks, even to mention in *detail* all the important observations that have been made within the past year. Clinical observers and laboratory workers have all been busy in enriching our knowledge of almost every department of biological science. The task of recording and harmoniously arranging these must be divided up among many specialists and placed where they can be intelligently utilized by the busy practitioner.

I shall, then, only attempt, briefly, to advert to some of the special points in which it seems to me that scientific work is most fruitful not only of pure knowledge, but of practical application; and is most likely to lead far into the regions, at present unknown and unexplored.

If we find that much of the present accurate scientific knowledge has been preceded by theory and vague presagings, that practice has sometimes been right before theory could exactly say why, it is no new thing.

Our perception of the value of hints and suspicions is wonderfully quickened when it is found by the careful worker that some of the thousands of guesses emitted have really hit the mark.

The birth of a new science, or at least, of the perfection of methods of research so distinctly separated off from all former ones as to call for its own laboratories, its own specially skilled workers, and which give us a kind of result hitherto unknown, is, to be sure, not an event of the last year but recent enough to be well within the memory of young men.

Bacteriology, the recognition of specific growths, not only by their form as revealed by the microscope, but by their behavior, their habits of growth, as we know from a distance a grove of pine trees from oaks or apples long before we can identify them by their botanical characters, is, undoubtedly, both in medicine and surgery, the most path-making of modern discoveries.

While it is true, that in both great departments of the healing art there have been, for years, dim pre-

sagings and theorizings of a germ theory, yet it is equally true that it is only recently that the various procedures, simple enough in themselves, have been systematized into a coherent and practically working whole, that consistent, definite, exact results have been brought about.

The surgeons can tell us that antiseptic surgery, as an art, began before bacteriology attained its position as an exact science; but, they will also say that the growth of both science and art has been going on in parallel lines, each confirming and improving the other.

And, in medicine, I think we may find that some of the older and less exact observations receive an explanation and are made more definite and decisive by the recent discoveries of bacteriology and organic chemistry.

The most beneficent of all specific forms of prophylaxis, that of variola was, as we all know, the result of a chance observation utilized by a careful and scientific mind entirely without the assistance of any modern instrument of precision except that one, without which all other laboratory apparatus is useless, the level head.

This single and signal success has been the stimulus to numberless attempts to imitate it in regard to other diseases of the same class, and will continue to inspire them until, with the assistance of the studies made more recently, success will be attained.

Close alongside of this study has come that of the organic substances, chiefly poisons, which is doing so much to enlighten our medical pathology, and which has received no slight impulse at the hands of the Chairman of this Section at last year's meeting.

The theory of autogenetic poisons, again, is not new. It is, perhaps, as old as medicine itself; but, when we are able to get the poison into the test tube and recognize its behavior with various reagents, we have assuredly advanced a most important step in our pathological knowledge.

The retention of various well known products has been long suspected as the cause of symptoms.

Bile must have been so as long as physicians have been aware of jaundice and its relation to the liver.

Urea has been tried and acquitted as harmless.

Uric acid is on trial, and evidence enough to convict it of a host of misdemeanors and, perhaps, crime is forthcoming.

But it is probable that we have yet to detect more guilty and better hidden criminals before we get at the real cause of uremic poisoning.

Fever is not, improbably, the effect of a poison, generated by inflammation, or by infection, which paralyzes the heat regulating centres. Whether the resulting rise of temperature is an effort of nature to counteract and neutralize the poison is a suggestion; but as yet, only a suggestion.

In cases of diphtheria a poison has been extracted from the membranes in two forms, an alkaloid and a poisonous albumen.

Similar poisons may be found in the blood; and, it is probable, that, in addition to the penetration of those formed in the membrane, renewed and continued formation may take place there under the influence of a special ferment. But the bacteria themselves do not go beyond the infected surfaces. All this has nothing in it startling to the clinician. It harmonizes with the views acquired by clinical observation, but it is a nearer and clearer view of what was before seen in clouds and uncertainty.

While we are bringing these accusations against the various pathogenic microbes let us, by the way, and as a digression, put in a plea of mitigation of damages.

It is not at all certain that many of the natural and physiological fermentations may *not* take place under the influence of bacteria.

Sterilized milk does not appear to contribute so actively to nutrition as in its natural state. It is, of course, less likely to carry disease; but, if it fails to carry health and strength, this negative merit is more than off set.

An extremely interesting series of observations has been made within a few years in regard to the appropriation of nitrogen from the air by plants.

It has always been supposed that vegetables derived their nitrogenous constituents from the decomposition of organic material in the soil or, if at all from the atmosphere, only indirectly and by way of ammonia.

It has been found, however, that some plants, among those actually experimented upon, the bean family in particular, are capable of growing in a soil absolutely free from nitrogen and of forming the usual nitrogenous compounds; thus, of necessity, deriving the necessary elements from the air. The power to do this is closely proportionate to the number of little tubercles or nodules found upon the minute rootlets of the plant, and these again, are dependent on the presence of a peculiar bacillus.

Thus, these lowly organisms that we regard almost wholly as pathogenic, control the first step in the process by which the vast stores of nitrogen in the atmosphere are transformed first into the albumen of the bean, and then, through this humble but intellectual instrumentality, into the highest cerebral tissue. So we must not condemn the whole family on account of the vicious habits of some of its representatives.

Among the most interesting of the secondary series of investigations which have grown from the original bacterio-chemical researches is to be placed that relating to immunity, showing under what circumstances and by what means the system is protected against infection.

It has been shown that the resistance to the invasion of a specific infecting bacillus is largely dependent upon the blood, although this by no means proves that the solids of the body may not have a share therein.

But the strife has waxed hot between those who think that infecting microbes are rendered harmless by the blood germs which may have attained peculiar qualities, or that they are swallowed by the leucocytes and thus arrested in their career of destructiveness.

Although the weight of opinion appears to be with the latter party yet there is no real reason why both constituents of the blood might not be active in the defense of the system.

This branch of the subject is as yet so very recent and so little settled that it will, undoubtedly, undergo modifications before becoming an integral part of acquired knowledge.

The immunity gained by various forms and successions of inoculation, that immunity of which the protection afforded by vaccinia against variola is the

type, is, of course, a specific one, a protection against some one disease and that only, and seems to depend upon the use of some product of the disease producing germ. So that the growth of the germ up to a certain point produces something that prevents its further growth, as the alcohol, which is the product of ordinary fermentation, when it has reached a percentage of sixteen, more or less, checks the further growth of the yeast plant and the process comes to a stand still.

This form of immunity, with the single grand exception just spoken of as the type, has not been certainly produced in man. Many attempts have been made to procure the same kind of immunity from other infectious diseases, notably, diphtheria, tuberculosis and hydrophobia. I include this last among the unsuccessful attempts with a certain amount of hesitation, as there are those who believe that it should rather be added to the other class.

The failure of the attempt of Koch in the same direction has been distinctly and definitely recognized only within the last two years. The disappointment was, indeed, great, but greater than it need to have been had the modest and careful student been allowed to follow his studies to their proper conclusion, and the methods of the newspaper correspondent and the advertising quack excluded from a region which should have been governed by the most rigid scientific precautions and unbiased judgment.

We cannot help recognizing that there are some defeats more honorable than some victories. The bold attack of the careful and skilful Koch upon the chief destroyer of our race, failure though it may have practically been, calls for our most sincere and fervent admiration. No one who had the opportunity to watch for himself the effects of tuberculin and compare them carefully with extended reports of others and with bacteriological investigation carried on in the same direction, could fail to be convinced that we had at least a substance and, so far as known, the only substance that bore a definite and specific relationship to the tubercle.

It is no wonder and no great harm, that the breeze of popular favor, fickle as ever, should have veered round as usual, but the infliction of anything like ridicule or reproach at the hands of scientific men, upon a piece of work so logical, so careful, so thorough, based upon a received and firmly-founded discovery of the same man, a discovery in itself enough to place him in the foremost rank of biological investigators, seems to me an act of ingratitude and cruelty.

It is by no means beyond the bounds of possibility that modifications of the method may yet do us good service, but let us hope that the work will be completed before it is carried from the laboratory to the public prints.

The other form of immunity is, in some respects, less complete but more general, *i. e.*, the resistance which a healthy organism offers to disease, to any disease. It is well enough known to all practitioners that some persons are much more prone to infection than others, with so far as we can tell, the same exposure and the same dose of poison.

We know that this is not always and entirely a matter of the health and strength of system generally, but it is, certainly, very often connected with it, and it seems to me that the extremists are sometimes in danger of forgetting this form of immunity in their desire to emphasize the danger of infection. I speak, of course, especially of tubercle. Within the last few years we have had the doctrine urged upon us that the spread of tuberculosis is due almost solely to the inoculation of the tubercle-bacillus. In one sense, it probably is so. That is, a person to whom no tubercular bacillus has access, will undoubtedly be free from the disease. But how many such persons are there living within the limits of the United States, or, in fact, in any reasonably thickly settled country in the world?

Yet we do not all have tuberculosis. The experienced practitioner, the observing clinician, and indeed the intelligent public have always clung stubbornly to the fact that tuberculosis is more likely to attack some kinds of constitution than others; the life insurance examiner cannot be persuaded by any theory of accidental bacillary infection that a descendant of a tuberculous family is as good a risk, no matter what the surroundings, as one from healthy stock.

It is impossible to account for the distribution of phthisis solely and exclusively by the opportunities for infection. There, certainly, must be a special susceptibility, congenital or acquired, a readiness of the soil for the seed.

There are many facts of resistance and vulnerability in the human subject which is not easy exactly to parallel in the lower animals, so that experimentation cannot give so conclusive results, but there are certain peculiarities as regards race and breed which are to some extent parallel.

There are some recent experiments which show how much and how rapidly the resistance of the system to infection can be increased and diminished by agencies which affect the general nutrition. The observers used as their test poison the charbon or anthrax (Milz-brand) bacillus, and their subjects were certain animals which are normally but little susceptible to this poison, pigeons, cocks and white rats. They found that if pigeons, which possess this relative immunity, receive the poison when in a condition of extreme hunger they lose their power of resistance and die.

If they have been starving six days before the dose and receive proper nourishment soon after they do not die. If the starvation period has been longer than six days they cannot, as a rule, be restored.

The authors conclude from the short period of time, which is necessary to produce these effects that the loss of immunity arises rather from the want of nutritive and resistant material in the blood than from any wasting of the solid tissues.

It is one of the commonplaces of medicine that diseases rage with special virulence among the ill nourished; years of famine are years of disease. The history of typhus, it has been said, is simply the history of human misery.

The dispensary physician recognizes the underlying diathesis of many of the affections he has to treat as poverty and hunger, and looks to the diet kitchen rather than to drugs for relief.

It has undoubtedly not escaped the attention of most of you how common it is to see a family of children go through with several infectious diseases in rapid succession, as, for instance, scarlatina, measles and whooping cough.

This illustrates well the difference between the two kinds of immunity. A child has scarlet fever. The development of the poison produces such a

change in the system that it is no longer susceptible to this poison. The disease is self limited, just as the fermentation ceases in the sugar solution when sixteen per cent. of alcohol, more or less, has been formed.

In addition to this the system, for some reason, is no longer capable, for months, for years, for a life time, of reacting to that particular poison. The patient has acquired a *specific* immunity. But while acquiring this *specific* immunity the *general* power of resistance dependent on the perfect nutrition of all the tissues, perhaps, especially the blood, has been decreased by the universally depressing effect of the scarlatine poison and the fever, so that to every other poison, beside the scarlatine the patient is more susceptible than before, and may then, in the presence of another poison, go though with the appropriate symptoms and acquire another specific immunity, say this time, as to measles. It is obvious how important such considerations must be in the matter of prophylaxis and therapeutics.

To go back to our original theme of tuberculosis, we cannot help recognizing, in the light of the more recent researches, the danger of infection to a considerable number of persons by the tubercle bacillus, whether in the milk or flesh of tuberculous animals, or that which is, probably, far more common, the dried sputum floating in the air and blown about everywhere as dust. This we can only to some extent avoid. Crowded rooms and conveyances and city streets are, of course, dangerous; but, how many can avoid them? Association with the tuberculous probably carries with it a certain risk (though, in my opinion, not a very great one), but affection and duty may be more powerful motives, even, than the dread of consumption.

Specific immunity from tuberculosis we cannot yet produce.

We find ourselves, therefore, with no lightening of the duty always imposed upon us in regard to tubercles, but with another added which bears not only on the physician and his patient's welfare but on the duty of that patient toward the community and that of the public health officers in the same direction.

The necessity of guarding against the tuberculosis of cattle is an important one. The phthisical patient should be made to comprehend the danger of spreading abroad the cultures of his bronchial tubes and cavities. But beside this, the demands of the old plan of getting the general nutrition into the best possible condition, either before or after the lodgment of the bacillus, is just as urgent as ever. The danger which cannot possibly be entirely avoided must be met by strengthening the defences. Fortunately, many of the same means, the most important, the most effectual, answer both indications. The air of lofty, dry plains is, firstly, relatively free from the infecting bacillus; secondly, a general tonic; and, thirdly, stimulates to increased vigor of respiration which renders the culture media of the bronchial tubes less appropriate for the growth of germs, should any find lodgment there. Until a prophylaxis dependent on a specific immunity can be obtained, it must be built up upon a general one; and, so far as possible, avoidance of the specific infection.

We used to be told that we should beware of treating the disease, that we should treat the patient who had the disease, and that disease was not an entity but a mere group of symptoms.

Now, looked at simply as a caution against a too vigorous and thoughtless therapeutics, this maxim does very well; but, as a fundamental rule of practice, it is one we are always striving to get away from.

Whenever we can get at the materies morbi we get rid of it, if possible, and it would, certainly, be highly unreasonable to do otherwise, provided, always, that the attempt do not endanger the patient more than the disease does. Thus, whenever we can we wash out the poisons of lead, mercury, arsenic and syphilis. We destroy or decompose or dissolve the rheumatic and gouty poisons with alkalies and salicylates.

We should do so with more poisons if we could get at them. Now, when the bacteriologist gets his seed from a certain disease, cultivates it, separates it from anything else, and then reproduces the disease by re-inoculating it, it seems to me that we have a very real and non-imaginary materies morbi indeed: and the attempt to kill or neutralize it is an eminently reasonable and proper one.

Unfortunately, human protoplasm is amenable to most of the same influences which act unfavorably upon organisms so far removed to the other end of the scale of being as the bacteria. In other words, most of our antiseptics and disinfectants are dangerous to the patient as well as to the disease germ; and, I fear, it will not be until the ideal disinfectant or group of disinfectants is found, that we shall be in a position to fight the germ without danger to the host.

It seems to me very problematical whether we are ever able to successfully attack the specific poison of most diseases after it has once fairly entered the circulation—that is, with drugs of the old fashioned kind. Some exceptions there are, already, to this rule. Our old friend, quinine, still holds its own and the plasmodie of malaria undoubtedly diminish and disappear under its influence.

And, again, it is undoubtedly, to a considerable extent, within our power and, probably, will be more so, to disinfect the intestines and other mucous surfaces which act as portals of disease, as the surgeon now does the cutaceous surface through which to make his incisions.

But, for most of the infections we shall have to wait for the special and peculiar results obtained from the bacillus, itself, which we desire to combat.

One paragraph in the rules of the Association demand that the Chairman of each Section should make suggestions in regard to the improvement in the methods of work.

Sidney Smith made a remark showing much knowledge of human nature, to the following effect:

"Benevolence is one of the most universal sentiments of mankind.

"A never sees B in distress without wishing C to relieve him." I might paraphrase by saying, "Scientific enthusiasm is one of the most common attributes of the physician.

"The medical orator never sees a good line of research without wishing the worker to follow it up."

There are many subjects upon which I should be glad to see laboratory work done; but, I am certainly not presumptuous enough to make any suggestions to the workers as to the direction or improvements of their researches. The founding of laboratories with all the appliances for following up the new lines of research mark an era in medicine.

We have reason to congratulate ourselves that edu-

cational interest and private liberality are doing for us in the United States a work which remains, to a considerable extent, at least, still in the hands of paternal governments in the old world, and that there is constantly growing up among us a class of men eager to enrich our science and capable of fully profiting by the advantages in their hands. So that clinical medicine and experimental research may go hand in hand, each making the other more careful and more accurate.

The experimenters are constantly furnishing to us data, sometimes of a more exact and scientific character than we clinicians are ourselves able to attain, data which may be of the utmost value in giving us new clues to the complicated tangle which we have to unravel in our study of human disease.

Our material and our methods, no matter how careful we may be, do not permit us the same kind of results or the same kind of precision which may be attained in the laboratory. On the one side the physician who wishes to work for the elevation of his calling, is required, as a scientific observer, to be accurate, to weigh and to count instead of to estimate and suppose, to correctly determine the amount and the location of the lesion, the height of the fever, the amount of the excreta, the species of the bacillus, the dose of the poison; then, also, as a sagacious practitioner, he must guage the factors which cannot be expressed in a numerical form, the weakness of tissues, the nervous instability, the diathesis hereditary or acquired and, finally, as guide, philosopher and friend, he has to allow for human frailties, the prejudices of family, race, religion, for vice and for ignorance, for interest, for caprice. Verily, the problems of the sick-room are not those of the laboratory.

It seems to me that the function of the physician, as an agent in the advance of biological science or a contributor to the physical welfare of society in the race, ought to approach as closely as the situation will allow to that of the laboratory specialist, and this is all I can offer as my suggestion as to improvements in the methods of work.

That clinical work should be freed, as far as possible, from the personal errors (amiable weaknesses, perhaps, but pernicious to the truth) into which almost every one of us is more or less easily led by ambition, the desire to excel, to originate, to feel that one has done a good or a great work.

That one should report what actually happens, not what he supposes the cause of success or failure to have been, not speculation but observation, and that he should be just as candid with failure as with success.

In other words, that the attitude of the physician toward medical *science*, not toward the patient or toward the medical *art*, should be as impersonal as he can make it.

Discussion.

Dr. David N. Kinsman, of Columbus, Ohio, in opening the discussion said: This subject of immunity is today one of pressing interest to the profession. I believe that any man who has anything to bring to the profession on this subject ought to do so, and, as I have been engaged in some experimental studies with a bearing in this direction, I desire to narrate the results.

The subject of protection is one thing, absolute immunity is quite another. We can by certain processes render a man partially immune; or we can by other processes render him more immune; but I doubt if we can render him entirely immune. As yet we have only the results of experiments from which to draw conclusions. No matter upon what animal we make the experiments, they have a bearing upon the whole subject.

The speaker then narrated how for some time he had been interested in making experiments in the production of immunity from hog-cholera by means of successive inoculations in the hog.

Dr. Didama, of New York, asked the last speaker whether any results could be obtained from the inoculation of fluid filtered from the cultures, or whether the inoculations have to be made with the bacilli.

Dr. Kinsman replied that they had not made an investigation of the effects of ptomaines and the other products of the bacilli, but that the inoculations had been made with the bacilli.

Dr. Dennison, of Colorado, arose to speak in regard to a statement which, he said, had emanated from Cincinnati. A physician in that city had stated that Professor Koch acknowledged to him that his tuberculin was a failure. After reading the statement the speaker wrote to Professor Koch to inquire as to its truth or falsity. Koch's reply states in the first place, that he has no knowledge or acquaintance with the physician who made the statement, and second, that the statement was utterly false. Tuberculin is not a failure; the failure is on the part of the medical profession. They have not come up to appreciate it. When they do, its success will be assured.

The speaker then referred to the culture experiments which have recently been made with the thymus gland as a culture-medium, by which the strength of the germs had been reduced so much as five or six thousand times. In this manner the poison of diphtheria has been experimented with. It is probable that the material which we use for the attenuation of virus will prove to be an important factor in the result which we attain.

Dr. Greenlee, of Kentucky, stated that he had had some experience with hog cholera, and that from his observations of its spread from a given locality to another, he had concluded that actual contact of the healthy animals with those having the disease was not necessary. He had often seen it spring up among hogs at a considerable distance from any known source of infection.

Dr. S. P. Kramer, of Ohio, remarked that he was happy to state that the individual who made the statement reported by a former speaker in regard to Koch's alleged acknowledgement that tuberculin was a failure was not a resident of Cincinnati but a visitor.

Dr. Kramer stated further, that in the production of immunity, two methods have been principally pursued. The one is that of the employment of attenuated cultures, as developed by Chauveau and Pasteur. The second is the method by which an albumin derived from the body of the germ is utilized. This, when brought in contact with certain principles contained within the animal organism, produces a body that neutralizes the toxic principle and reduces the pathogenic germ to the level of an ordinary saprophytic germ. In the first form, the antitoxin is formed by the growth of the germ within the body; in the second by the growth of the germ in the test tube. The first method, which may be called serum-therapy, has been credited with the cure of six cases of tetanus.

The next in order was a paper on

THE ETIOLOGY OF SPECIFIC DISEASE.

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Mr. Chairman and Gentlemen:—If, in the present paper, I take issue with any of the audience in regard to recent or novel theories relating to the proximate causes of our most important diseases, be assured that it has been done with all sincerity, and with the kindest feelings for those with whom I may differ. The writer does not wish to be considered as actuated by a spirit of obstinacy, or unreasonable skepticism. Indeed, it is so much easier to accept the dictum of others, or to shift the responsibility of our views onto those who set themselves up as authority, than to formulate the lessons of our own observation and experience, so much more convenient to accept the thought of others, than to think for ourselves that natural indolence protests against the sacrifice. In medicine, as in all other pursuits of

life, we are more inclined to simply acquiesce than to contradict, and usually there is so little encouragement to do otherwise that we rarely wish to intrude our own opinion, but infinitely prefer that others may take the lead and we will follow, however fallacious and misleading such guidance may prove. Hence, it may be said to-day, as in the past, the greatest bane to medical progress is slavery to so-called authorities, and that one of the greatest hindrances to the acquisition of real knowledge is a blind reverence for great names.

Asking your pardon for these introductory and somewhat irrelevent remarks we will now call your attention to the subject of our paper, namely, The Etiology of Specific Disease. By specific diseases is meant certain maladies possessing distinct characteristics, and which are not only engendered by special causes, but are supposed to be incapable of development without the application of a so-called specific cause for each disease respectively. Such, at least, is a strict definition of the phrase "specific" as warranted by the phenomena of infectious and inoculable maladies. According to our text-books there are 1,146 diseases which affect mankind, and require the study and attention of the physician, but from this extensive nosological list, less than twenty-five diseases are to be reckoned as belonging to the class under consideration. Yet these are known to cause nearly two-thirds of our total mortality. Medical history informs us that many of the pestilential maladies which scourged the world in past ages belong to this class and far exceeded the mortality of any which now prevail. Indeed, so great was the special increment of their spreading power and malignity that as *Niebuhr* has shown they not only decimated fleets and armies, but influenced the fate of cities and empires.

On the great plains of plague-stricken Asia, centuries before the Christian era, the query: "Shall such ills come by chance?" was then answered—

> "Like the sly snake they come
> That stings unseen; like the striped murderer
> Who waits to spring from the Karunda bush,
> Hiding beside the jungle path; or like
> The lightning striking these and sparing those,
> As chance may send."
>
> —*Light of Asia.*

Shall the intelligent physician of to-day meet this problem with no more rational interpretation than the ancient Buddhists in the earliest dawn of the world's history? Has the acquisition of long experience or the accumulated knowledge of the past thrown no light upon the cause and prevention of the maladies under consideration? In answer to this it may be said that the proximate cause of "specific" disease is now as it has ever been one of the most puzzling questions with which the human mind has had to grapple. It can not yet be said that we have positive knowledge as to the specific poison, if we may so call it, which produces scarlatina, diphtheria, yellow fever or cholera. The chemist can not detect in the atmosphere the cause of those infectious diseases which spread only through this medium, or chiefly in this way, and to assert that he can with certainty detect any peculiar substance in the blood of the most pestilential malady, that is its *positive etiological* factor would be a statement in advance of the facts of exact science. Neither by the microscope, nor by the minutest chemical analysis can we distinguish the pus globule of small-pox or of syphilis from the most laudable pus of the surgeon. Nor have the most delicate tests as yet shown any thing especially distinctive in the saliva of a rabid animal by which a specific disease of this class is communicated so certainly and positively by direct contact. The *agens morbi* of these diseases in our present state of knowledge is still an unsettled problem. But since it has been truly said that the "curse causeless shall not come" some explanation of the etiology of our specific diseases, like Banquo's ghost will not down, hence modern physiological, pathological, microscopic and chemical research has led to the presentation of several novel and plausible theories to account for the proximate and specific factors which give rise to the development of the diseases known to be of infectious or contagious character. Before their consideration, however, it may be said that it seems impossible to explain the development of any specific disease either *exopathic* or *endopathic* unless there be a recognition of certain predisposing causes of such maladies. We refer to pre-natal, conditional, and functional influences. There can be no reasonable doubt that some of the so-called specific diseases owe their origin mainly to a hereditary transmission of a proclivity to such disorder. This is notably true of tuberculosis, or it may be supposed that the inheritance lies in the tissues or tissue elements predisposing to certain diseases in certain families. In the spreading of epidemics, contagious and personal, susceptibility may be factors in a partly *conditional* sense. Influences which the old authors called "atmospheric," the various direct and indirect influences which relate to the normal succession, and occasional abnormality of seasons in respect to the isolation of our planet and of the temperature and humidity of air and earth are perhaps generally too vaguely regarded as elements of interest in the present question, but are possibly factors which no one who tries to solve these problems should omit from scientific consideration. Again, failure in the function of the lungs, the liver, the intestinal glandula, the kidneys and the skin to eliminate the *waste products* of the system must be regarded by the physiologist as one of the most potent predisposing factors in the production of every form of zymotic disease. Science is more and more teaching us that the "survival of the fittest" is applicable to specific disease, and that the victory will be on the side of the attacked in direct proportion to the normal condition of all the bodily functions, and that the factors mentioned not only favor the development and intensity of such diseases, but that without such influence their establishment would often be impossible.

The theories of the proximate and specific cause of specific diseases may now be noted in the order of their popularity, but inversely as we believe with respect to their true etiological import. We refer to the *bacterian* theory, the *bioplast* theory, the *chemico-physical* theory, the *nervo-glandular* theory, and the theory of *perverted vital force*. The first hypothesis attributes specific diseases to the agency of microbes or minute living objects. By most authorities these are all classed in the vegetable kingdom, and might be termed microphytes of the fungous order. By some they are dominated parasites. They are supposed to operate by producing changes in the structures affected— the blood, the cutaneous and mucous membranes, the cellular tissue, and glandular organs of a destructive character, deranging their functions,

disturbing the processes of nutrition, of circulation, of calorification and secretion. Fever, loss of appetite, emaciation, prostration of the muscular and nervous forces being the usual results. Their mode of action suggested by the belief that they are living objects, is that by enormous multiplication they may act mechanically through their bulk, obstructing the capillary circulation, and by pressure on solid tissues cause their gradual destruction by robbing the blood of the pabulum used for growth of the mycrophytes, thus effecting emaciation by spoliation, and developing fever by conversion of molecular motion into heat, and deranging secretion by mechanical intrusion in the glandular structure; also engendering neurosal affections by similar action on nerve structure.

The discovery of the dependence of alcoholic fermentation upon the presence of the yeast plant (*saccharomices cerevisa*), and the general resemblance between the symptoms of contagious maladies and the processes observed in the fermentation led to the use of the terms *zymosis* to express the action, and of *zymotic*, to express the character of all those diseases to which microbes in general are supposed to give rise. But all such views for the present must be largely speculative. There are many points of difficulty requiring to be more fully illuminated by careful observation before the bacterian theory becomes an established doctrine in etiology. Many of these difficulties have been well formulated by Professor Hartshorn, of Philadelphia, and may be expressed in part, as follows:

1. Throughout all the investigations which have been made, or likely to be conducted, there remains the extreme difficulty, if not impossibility, of total separation between the microbes themselves and the matter of the vehicle in which they exist, such as blood virus, vitiated secretions, artificial culture material, or whatever it may be. All the effects ascribed to the bacteria, except their proliferation and mechanical intrusion, may, with equal propriety, be attributed to the toxic action of a portion, however minute, if the soil in which they have lived, whose modifications must be coincident with those which they undergo.

2. The absence of the characters belonging to definite organisms in the easily studied virus of smallpox and vaccinia is presumptive evidence against the probability of such organisms being essential to the causation of other enthetic diseases.

3. Bacteria are rarely seen in the incipient stages of disease, but after the blood has become impoverished, the secretions depraved or morbid products are undergoing decomposition they are found most abundantly, and are found most numerous in materials of a septic or infectious character after their period of toxic intensity has passed by.

4. Bacteria have been, however, sometimes abundantly discovered in healthy bodies upon the various mucous membranes, in the blood and it is said in countless number in fecal discharges without any specific disease following.

5. Suppuration may be produced without the presence of minute organisms of any kind. Bacteria have been found under Lister's antiseptic dressings without suppuration following. Pathological investigators (Paul Bert and Rosenberger) have destroyed all the microbes in a septic fluid and yet found it to retain its poisonous quality. Various elaborate investigations have proved that fatal septic poisoning can be produced in animals by the products of decomposition without the presence of living organisms, and experiments have shown that normal blood, when deprived of oxygen, in the absence of microörganisms, may acquire *septic* properties, and also that septicæmia may be induced by the injection of free fibrin ferment and other substances into the blood in the absence of such minute organisms. The same condition has also been produced by the subcutaneous injection of filtered saliva containing no microbes.

6. While Klebs and Koch maintain the definite *specificity* of each minute microphytic organism, on the contrary, Billroth, Burden, Sanderson and others assert their mutual convertibility according to the influences of environment, and Pasteur, Wood and Formad report experiments making it appear that modification by culture is possible, converting an innocent into a malignant parasitic organism, or a death-producing microbe into one capable only of causing a transitory and not dangerous local affection which, nevertheless, secures to the animal thus treated immunity when subsequently exposed to the deadly infection. But in none of these cases is there reported any morphological change, whatever, in the bacilli or micrococci experimented with; their capacity of reproduction through several generations being retained.

7. Other points of objection to the bacterian causation of disease relate to their specificity. While it may be conceded that like produces like is a rule of nature, and that different forms of bacteria may reproduce themselves, it does not necessarily follow that they can reproduce the disease which they may accompany. That disease may be propagated like plants and animals, by means of a peculiar form of these organisms would require the recognition of disease as an entity and not a physiological perturbation, but the analogy is so absurd and the assumption so unwarranted as to need no argument for its refutation. While this theory requires the belief that each specific disease is produced by a certain variety of these organisms and no other, it fails to explain *how* and *why* they are causative of a special disease or of immunity against subsequent attacks. If they act as a specific cause of a specific disease by their enormous multiplication in a mechanical way, through their bulk obstructing capillary circulation, or by pressure on solid tissues causing gradual destruction (as in tuberculosis), this is a property that is not confined to any particular form of such organisms, but is common to all of them. Then why should one particular variety engender one disease and not another, and why should they not continue to produce identical results as often as they gain entrance into the system?

It is held by Professor Jaccoud and others that the bacteria of infection are indistinguishable from harmless ones except by their effects, and that as the liquids inhabited by them are frequently infectious, they are therefore merely a medium through which contagion acts. In some instances becoming so infected themselves as to transmit the property through several successive generations. Again, some observers attribute the symptoms of many acute infectious disorders to rapid development of poisons similar to vegetable alkaloids by bacteria in re-moleculizing the normal fluids. Others hold that such animal alka-

loids are constantly produced in the living body by albuminoid decomposition without such agency, and that the general function of minute cryptogamic organisms, when present, is of a beneficial or conservative nature in re-appropriating the product of organic decay.

Analogy in nature renders this scavenger theory more probable *per se* than that which holds them to be destructive parasites or poison-producers in the bodies which they may inhabit.

We may next briefly consider the vital germ theory, of which Lionel Beale is the chief exponent. Dr. Beale, than whom no greater micrologist has ever lived, uses the term *bioplasm* to designate the physical basis of life and growth. This consists, according to his views, of separate particles of less than $\frac{1}{10000}$ of an inch diameter, originating in the blood, and designed for the nourishment and growth of all the tissues of the body. They are described as soft, without color or structure, and enclosed in a colorless capsule, through which liquid pabulum passes for their growth. New bioplasts are formed by divisions of mature ones, and the new ones continue to grow by imbibition until they divide or contribute to the formation of solid tissues. *Microphytes*, with an average diameter of $\frac{1}{90000}$ of an inch, are considered by him as the lowest form of bioplasm, existing in all the fluid and solid tissues of both plants and animals, as well as in all kinds of mineral substances, and under all meteorological conditions (though dormant under some conditions of temperature and dessication). Being omnipresent, and, as he believes, indistinguishable from each other by any precise physical characteristics, he denies their relation to disease of any kind. Contagious diseases are attributed by him to degraded or perverted bioplasm descended from original healthy bioplasts. These constitute what he terms "disease germs," which have property of self-multiplication like healthy bioplasts, both within the diseased body and in any healthy susceptible body to which they may gain admission. These contagious bioplasts are extremely minute, having a diameter less than $\frac{1}{100000}$ of an inch, and though possessing "specific" characteristics for every disease, one cannot be distinguished from another, either by the microscope or by chemical analysis; neither can the healthy bioplast be distinguished from the diseased by any test except its effects. The disease germs referred to in this connection become noxious only after entering the blood and then passing into the solid tissues and secretions. According to this investigator, their multiplication in the body always elevates the temperature, and this may continue after the death of the victim, and that fever is due to this process, and not to oxidation of tissues; death being the result of change in the composition of the blood and derangement of capillary circulation. Beale's theory of migrating or transplanted bioplasts, in the writer's opinion, contains an element of truth, but has thus far received very little support besides that of its distinguished author.

On the supposition that *disease germs* are only abnormalities or deviations from healthy bioplasts which may be detached from one body and planted, while yet retaining vitality, upon another, and which may there undergo changes more or less morbid and destructive to the individual by whom they have been received, we certainly have a more plausible explanation of the transmission of contagious disease than that which is claimed by bacteriologists.

We will now call attention to the *chemico-physical* theory of Liebig, which embraces the doctrine that the *materia morborum* may consist merely of inorganic elements or compounds which, by entering the body and acting as chemical poisons, engender specific disease, and which affirms that the action of a *virus* is not essential to the development of a zymosis, or fermentation in the human economy. This hypothesis has been more clearly expressed in the phraseology of the late Dr. Snow, of London, as the theory of *continuous molecular change*. Chemists have defined this change to be decomposition by contact, or the action of presence. An illustration of this law is the power which small quantities of certain substances possess of causing unlimited quantities to pass into the same state. The phenomena of crystallization, the molecular motion that takes place in the operation of skin grafting, the diffusion of heat from molecule to molecule, or the extension of a flame from a burning body to combustible material within its reach, may be cited as physical instances, and analogies of the operation of this law. Hence, if a decomposing organic molecule is introduced into the human body, by this law of catalysis or induction it imparts its motion to other molecules with which it may come in contact.

The processes in fermentation, putrefaction, septicæmia and the multiplication of small-pox or syphilitic contagion from the smallest inoculation in the human body, are accounted for in a similar manner. Against the necessity of the action of minute living organisms to produce these morbid processes, the advocates of this theory urge that the above named changes, and many others like them, are produced in the absence of such organism by chemical agents formed in the body, such as *leucomaines* and *ptomaines*, those physiological and putrefactive alkaloids recently investigated by Vaughan and others, and that inorganic substances may develop such changes, similar to the action of sulphuric acid when it changes starch into sugar. In support of this doctrine it may be asserted, that the bacterian theory that every particle of contagious matter is (at one time at least) a living organism, and that only such living organisms reproduce their kind and the diseases which they accompany, is one which loses weight as an argument in view of the natural history of small-pox and analogous diseases.

The Nervo-Glandular Theory of the origin of specific disease has been plausibly urged by Dr. W. B. Richardson, of England, and is apparently an outgrowth of his studies of the above doctrine of Liebig concerning fermentation and its relation to nitrogenous material. He was convinced by experiments that zymotic disease could be communicated from one animal to another by inoculation of various secretions. He also succeeded in producing from such fluids alkaloidal substances of crystalline structure. Inoculation of these in solution was followed by the same specific disease as had yielded the alkaloids. (Leucomaines? or ptomaines?) Hence he concluded that any animal secretion might be made to yield a contagious principle to which he gave the name "septine," and the maladies thus engendered were designated by him as "septinous." The true *contagia*, in his belief, are therefore all of glandular origin, and the venom of serpents was suggested as a type of their source and action, the effect depending not on a multiplication of germs, but a catalytic

influence, the agent changing other substances without undergoing change itself, and that the poison, therefore, is reproduced only in the infected and diseased body through its own secreting organs. He believes, also, that ordinary secretions may change character, and become poisonous without previous infection. For example, the exudation of ordinary peritonitis may give rise to puerperal fever, and typhus fever may be produced in overcrowded apartments by absorption of animal exhalation, and in this way contagia of various kinds may constantly arise *de novo*. In furtherance of his theory, Dr. Richardson emphasizes the fact that the number of separate communicable maladies has a close relation to the number and character of the secretions. As examples, hydrophobia is derived from the saliva of rabid animals; glanders from nasal mucus; enteric fever is traced to the intestinal mucous glands; diphtheria to the mucous glands of the throat, and scarlet fever to the secretion of the lymphatic glands, but admits that in some instances the blood corpuscles become the seat of the catalytic change. As Richardson maintains that communicable disease may arise without intervention of contagious matter, he supposes that the virus may arise through previous impressions upon glandular organs, and refers the origin of such cases to fear or anger, or other emotional disturbance, when no mode of communication can be discovered. In favor of this hypothesis much might be said.

It is now known that some of the most remarkable pathological effects may be artificially induced, either by drugs, the precisely localized and measured action of heat and cold, or by other agencies acting upon the nerve centres in the brain and spinal cord. And since it is admitted that the brain is not only the instrument of the mind, but that it presides over and controls the functions of all the other organs, its own disorders therefrom can hardly fail to affect them. Strong mental emotion may not only suspend or pervert particular functions, but is even capable of destroying life by arresting the action of the heart. Sudden mental worry may excite dangerous interference with digestion or start an abnormal cardiac rhythm. Mental shock can check or increase the action of the kidneys, and in fact affect all the secreting or excreting organs of the body. The influence of continued mental anxiety and the pernicious effects of habitual grief upon the nutritive functions, are plainly marked. Under its corroding blight the skin loses its freshness and grows dry and yellowish; owing to derangement of the liver the bowels become confined, and their habitual constipation is apt to be followed by absorption of fermentative and putrefactive gases and other noxious materials, giving rise to fecal toxæmia with all its consequences, and thus not only by reflex influence of local irritation, but direct influence through the blood, the vicious circle is completed by the further induction of disease of the brain and nervous system. Anger often brings on a convulsive attack, and insanity frequently follows close upon exaggerated mental effort, and especially upon violent mental emotion, whether of terror, grief or joy. The principle of moral contagion cannot be denied. The mind is affected by imitative influences. Thus chorea is excited in some individuals by watching choreic movements in others, and a single hysterical patient may arouse in others symptoms almost identical with her own, while the direct influence of the mental state upon existing disease, and in governing the susceptibility to others or favoring their development, is of the most potent character. For evidence of this influence in the genesis of specific disease the reader is referred to that most interesting book of Dr. Tuke's, entitled "Influence of Mind Upon the Body." Apropos to this subject are also the recent remarks of Sir Joseph Fayer, at the Sanitary Congress at Brighton, England, with regard to the expected invasion of the country by cholera. After denouncing quarantine and cordons as antiquated, worn out and obsolete devices, he urged that the true way to protect ourselves from this disease is to see that our homes are clean, that the water we drink is pure and the food we eat wholesome, and above all else *to keep our minds free from panic*. A panic state implies a disorganized vitality, and of its influence in aiding the class of diseases to which cholera belongs there can be no doubt.

GENERAL CONCLUSIONS.

Theory of Perverted Vital Force.—We must be somewhat brief in presenting the following conclusions as to the etiology of specific disease, as our views have been already emphasized as occasion occurred during our argument. It now remains for us to deduce several general facts which may serve to harmonize all of the theories presented, conceding to each its due importance, and out of all endeavor to construct one of our own which may serve to show how and why communicable diseases are made *specific*. The advocates of the bacterian, the bioplastic, the chemical, and of every other theory of zymotic disease, unanimously concede the fact that the presence of nitrogenous matter in a decomposing or readily decomposable state affords the best possible *pabulum*, either for the development of microphytes, the infection of bioplastic elements, the elaboration of animal alkaloids or the action of ferments. Hence a *common condition* which all these agencies require for their action in the production of specific disease is the presence of an excess of such pabulum in the blood of the individual attacked. Again, a careful study of the foregoing investigation as to the cause of the diseases under consideration certainly teaches that we must be on our guard against ascribing a specific etiological influence to the various forms of vegetable microörganism. For in certain cases these may have been in the first place non-existent, as when such a disease has been "autogenetic," and in no sense a derivative of antecedent disease of the same kind. This caution is especially applicable in regard to such an affection as erysipelas, which, although contagious, is also, on very good grounds, judged to be generable, especially during certain states of lowered health, induced by renal disease and some other visceral affections. Though not so positively known, it is by many deemed probable that a similar caution may be necessary in regard to more general contagious affections, such as diphtheria, typhoid and typhus fevers and cholera, which, though certainly infectious, may also be autogenetic. Among these diseases we might still mention several others which, although their ordinary or normal mode of spreading is by contagion, yet beyond reasonable doubt do sometimes arise spontaneously. We refer to such maladies as scarlatina and yellow fever, gonorrhœa, rabies and glanders; the two last, in fact, being only of spontaneous origin in the lower animals, from which they are communicated to man.

It would appear from the conclusions of Bastain and others, that in those complex, prolonged and continuous morbid processes constituting the phenomena typical of some particular infectious malady, that at some stage of this complicated chain of processes, and somewhere (that is in some organ or tissue, or in the blood) certain organisms may arise *de novo* and are not to be regarded as direct descendants of preëxistent organisms any more than we would regard the pus corpuscles met with in a case of purulent ophthalmia or gonorrhœa as direct lineal descendants of those which may have taken part in occasioning one or the other of such diseases. But admitting that the doctrine of *heterogenesis* is established and that of *archebiosis* or spontaneous generation is disproved by the experiment of Tyndall, it is by no means clear that the assumed mode of operation of microphytes in the causation of disease is the true one, or that their influence in the transmission of disease is not simply that of carriers of contagion the same as the non-vitalized chemical compounds of Liebig, the leucomaines and ptomaines of Vaughan, or the cast-off and altered glandular secretions and tissue elements of Richardson or Beale. It is not yet possible to say with regard to metabolic contagion what is the *essential constitution* of contagious matter, or what is the intimate nature of the transforming power which the particle of such matter exercises on the particles which it infects. Nor are we able, by actual demonstration, to say that contagion is a *material substance*. We know that the ancient philosophers in investigating the nature of heat regarded it at first as a kind of subtile matter which insinuated itself into the substances of bodies and resided there with greater or less manifestation of its presence, but heat is now regarded and proved by scientific observers to be, not a material substance, but simply a *condition of matter*—a phase of force, or molecular motion—and from the nature of its action *contagion* like the *force caloric* is, in the writer's opinion, a mere condition of matter and not a *material substance*.

As regards the question of the form of force which may explain the transforming power of the contagion of specific disease, science is still ignorant. Yet expert chemists express clearly enough the conviction that there exists a certain great unit of force in nature which lies beyond their power of analysis, measurement, or even of definite nomenclature. But in that most interesting, yet most difficult and hitherto almost uninvestigated branch of chemical dynamics, we are supposed to have our nearest clew to the scientific problems connected with the specific etiology of disease. Any theory which tends to explain the *rationale* of the processes under consideration must recognize the existence but perverted operation of the so-called vital forces. The theory which we present assumes the identity of the physical and vital forces. The physical forces embrace magnetism, chemical affinity, heat, electricity and motion. The vital forces are assimilation, combustion, animal heat, nerve force, and muscular contractility. All scientists now concede the correlation of the physical forces, that they are all convertible the one into the other, and the force, like matter, in any form can neither be created nor destroyed, and as presented to us in the universe, they are both indestructible and inseparable, perpetually existing, and unchanging in quality, yet ever changing in form. The intimate nature of force, however, is the greatest mystery of all unrevealed phenomena, visible only in its effects as manifested to our senses, it becomes at once an unknown and unknowable power, transcending all human knowledge and conception. We can only judge of its presence, therefore, by the peculiarity of its action, and the effects which it produces. If we accept the teaching of modern science, all matter is the vehicle of change, motion the result of change, and *force* the cause of change. Life, as we understand it, depends upon the presence of a material substance operated upon by force, resulting in movement, and the harmonious interactions of these conditions when applied to the animal body not only constitute life, but health, while its derangement as surely eventuates in disease and death. According to the demonstrations and conclusions of modern investigators of physical science, the *vis viva*, or life force, is simply the combined influences of the physical forces which are constantly changing in form during the various vital processes, the supply and action of the same being maintained by the food we eat, the fluids we drink, and the air we breathe. Let us suppose, for illustration, that the nutrient fluid charged with oxygen is placed in an *electro-positive* condition, at the same time the tissues are in an *electro-negative*, or magnetic condition, by which assimilation or chemical affinity is induced; this involves oxidation, combustion and molecular motion. Molecular motion is converted into (animal) heat, and heat is converted into (animal) electricity, or nerve force, and nerve force induces muscular contraction or mechanical motion, which in turn serves to assist and perpetuate the operation of the other manifestations of force, in that it maintains the respiratory function, contracts the heart and arteries, propels the blood to all parts of the system, and thus supply tissue waste and equalize temperature, as well as control the various secretory and excretory functions of the body.

Such are the different manifestations of the so-called "vital forces," the harmonious and normal operation of which constitute life and health, but when perverted will not only occasion disease and disorganization, but death either local or general, as conditions may determine. For example, if the blood from any cause becomes contaminated or deficient in oxygen, the forces governing nutrition, such as assimilation and combustion, will be perverted in their operation, waste materials, or materia-morbi will be developed, which may eventuate in morbific effects, either as irritation of nerve centers governing heat production, or local irritation exciting inflammation of various tissues or organs, as well as malnutrition and disorganization of various degrees and variety according to the extent of toxæmia, and the functional activity of those organs provided for elimination. For the materia-morbi thus accumulated may remain in a latent condition until equiliberation is commenced by increased oxygenation, and this may augment the amount of animal heat within the body, causing fever, which may in its turn induce pathological lesions, varying in character with its intensity, ten degrees of which mark the difference between life and death. Although force can only manifest itself by molecular motion, yet it may exist in two general forms known as potential energy and actual energy. Force stored up in certain conditions of matter, as in the tension of the particles of an explosive compound, such as nitro-glycerine, or in com-

bustible materials as wood, coal and the food of animals, is known as potential energy, that is, power capable of being liberated for the production of effects. But when the nitro-glycerine explodes, the fuel is burned, or the food is oxidized in the animal body, the force they contain is given out in the form of effects produced, and the potential energy becomes actual energy, or in animal bodies, living force. Such is the nature of unoxygenized material in the blood constituting an *agens morbi*, in that it represents potential energy, becoming actual energy, and capable of producing morbid effects when subjected to zymotic action.

Force acting upon different forms of material substance will manifest itself in different ways, as chemical affinity, combustion, electricity, etc. Also force in its different forms acting upon the same material substance may give to a multiplicity of effects, as quantity and local conditions may determine. But force of any particular form, whether physical or vital, operating in a certain direction, producing certain results, *tends to continue its action in that direction, and the production of the same results as long as conditions favorable to its action obtain.* Thus the molecular motion imparted to a conducting wire from a galvanic battery may continue for thousands of miles. A spark of fire may destroy a city, and so the smallest quantity of chemical or perverted vital force arising from the blood in a state of zymosis, and conveyed by means of its own elements, may set up the same morbid action in other individuals whenever their blood is of a suitable zymotic condition, and it is operation of this law that gives us the *rationale of contagion.* But we find in the physical forces, so in the vital forces; as in the great laboratory of nature, so in the individual organisms, that action is met by counteraction, and the force, however manifest, sooner or later tends to equiliberation. For this reason galvanic batteries become exhausted, fires must be fed with fuel, and zymosis ceases, and disease ends in the affected individual, and in communities when the material suitable for its action has been extinguished. Strictly speaking, therefore, *contagion is a phase of perverted vital force*, and this morbid influence does not imply the agency of microörganism any more than that of any other medium, vital or otherwise, which may serve to convey its action. The essence of contagion is not a material substance, nor does it necessarily require a definite agency through which it must operate, but is simply a form of force as imponderable in its nature as heat, light or electricity. Specific only so far as it naturally tends to operate in the same direction upon the blood of another individual when it contains certain constituents of identical character with those upon which it has been operating in the blood of the infecting individual, which is likely to be the case when individuals and communities are alike subjected to the same general and special predisposing causes of disease.

In conclusion it may be affirmed and reiterated that the agency through which contagion acts is not limited to any particular form of mycrophyte, or to the bioplasts of Beale, or to the waste products incident to tissue metamorphosis, for all may serve as carriers of a perverted vital force. While the types and varieties of infectious disease may be determined by the impressions made upon the nervous system, but influenced by the condition of the individual with reference to his powers of constitution, age, susceptibility, weakened condition of certain organs or tissues from previous disease or tolerance from like causes. And where the initial factor is due to accumulation of "waste products," then also by the character and degree of the defective excretion, and the route or channel by which the vital forces attempt its elimination. And finally, since any of the elements hitherto described as factors of contagion are infected through defective excretion, it necessarily follows that the character of an infectious or contagious disease depends upon the variety or nature of that glandular excretion which is most defective, and as these infected elements are most prone to elimination through those glands whose defective function has produced the blood contamination, we are thus afforded the *rationale* of the glandular involvement of specific disease. And if any such morbid elements were still under the dominance of the natural laws of *elective attraction* which appropriates from the blood certain elements to certain tissues or parts of the body, or that normal *repellant force* peculiar to certain excretory glands, their *deposition* in these localities would be secured, or their ultimate *elimination* (if the patient survived) through those glands and parts of the body identical with those from which they have been derived would be effected. And thus again would be stamped upon the disease its "specific" character.

Owing to the great length of the paper, only a part of it could be read, and it was passed without discussion.

(*To be continued.*)

SOCIETY PROCEEDINGS.

Gynecological Society of Boston.

Regular Meeting, held June 9, 1892.

THE PRESIDENT, AUGUSTUS P. CLARKE, M.D., IN THE CHAIR.

(*Concluded from page 52.*)

[25] Children's Hospital: The catgut is first scrubbed with yellow soap, and allowed to stand in ether 48 hours, in corrosive (50 per cent.) 48 hours, and finally preserved in the following solution:

Corrosive sublimate, gr. xv;
Glycerine, oz. ijss;
Absolute alcohol, oz. xxxj.

[26] Brunner scrubs the raw catgut with strong potash soap, then places it either directly in corrosive sublimate, 1–1000, or after leaving it one half hour in ether. It is finally stored in the following solution:

Corrosive sublimate,	1 part.
Absolute Alcohol,	900 parts.
Glycerine,	100 "

To test the aseptic condition of gut prepared in this way, Brunner manufactured gut from the intestines of an animal dead of anthrax; some he prepared and used without producing trouble, whereas the raw gut when inoculated into animals, caused fatal anthrax in all cases.

[27] Roswell Park immerses the catgut in benzine or ether; it is then dried and soaked in 1 per cent. sublimate solution for one or two days, again dried and placed in juniper oil and finally preserved in alcohol containing corrosive sublimate 1–1000.

[28] Bilroth's Clinic: Catgut is first washed with potash soap, it is then laid in ethylic ether twice for 12 hours each time, then dried and sterilized in a dry chamber by raising the temperature to 250° F. Then it is immersed in a solution of

sublimate 1–1000 for 24 hours; it is kept ready for use in absolute alcohol.

[29] Wm. Goodell: The prepared catgut comes in greasy coils of a dark amber color. To dissolve out the fat these are placed in commercial ether for from 24 to 48 hours, according to the size of the gut; and if the gut is of the larger sizes the ether is changed once. The gut is now immersed for 48 hours in a 1–1000 alcoholic solution of corrosive sublimate. It is then wound on glass spools by surgically clean hands, and kept permanently for use in a mixture of 2 parts of oil of juniper to one of alcohol, which is occasionally changed. When needed for an operation, I transfer the requisite number of spools to a mixture of one part glycerine, which has been sterilized by heat, to 9 of alcohol.

This gives a greater smoothness and pliability. Thus prepared it will last in the tissues of the body from a week to ten days.

A study of these methods shows the preparation of catgut to consist of several processes.

1. For removing fat, such as scrubbing with soap, and soaking in ether, benzine, or bi-sulphide of carbon.

2. For sterilizing, by dry heat, boiling in alcohol or immersion in antiseptic solutions, or corrosive sublimate, chromic acid, carbolic acid, alcohol or juniper oil.

3. For tanning or altering the consistency of the gut so that it will resist the absorption by the tissues for a longer period, as the soaking in solutions of chromic acid.

4. For rendering soft and pliable, as by the treatment with glycerine.

5. For preserving; storing in solutions of carbolic and oil, juniper oil, alcoholic and watery solutions of corrosive sublimate, and absolute alcohol.

No one method comprises all of these processes, and therefore they may not all be essential for the perfect preparation of gut. Removal of the fat is necessary when the gut is to be sterilized by dry heat but not so in the other methods. If the natural oil of the suture is removed, it is usually replaced by storing the gut in an oily preservative fluid. The removal of the fat, may facilitate the sterilizing of the gut, and at the same time make it less irritating to the tissues, for the same reasons that ointments are abandoned in the dressing of wounds. Fat is not essential to the perfection of a suture, and therefore it may as well be removed.

Sterilizing by dry heat is a sure method; it requires more care; the gut is not so strong as that prepared by other methods. Boiling in alcohol requires less heat than in the dry method, and the tissue is hardened, both by the action of the alcohol, and heat. It is thus made stronger. Soaking in aqueous solutions macerate the tissue, tend toward their disintegration, and thereby weaken them, at the same time it enables the antiseptic substances used to penetrate deep into, and through every part of, the sutures, rendering them aseptic. Lister's method, which has proven quite effectual, owes much of its success to the action of the water in first softening the gut so that it can be thoroughly acted upon by a minute quantity of chromic acid.

Tanning, or hardening of the gut by chromic acid, is one of the processes of decided value, since it renders smaller size gut as efficient as larger sizes prepared in other ways.

Marcy uses Lister's process entirely, whereas Bryant objects to the method, saying it makes the gut resist absorption too long. It must be obvious to every surgeon that there are many instances where it is of decided advantage to have this suture resist absorption. Naphthol acts as a tan. Anything that will increase the pliability of a suture without diminishing its strength must be considered an advantage to its preparation. All animal sutures kept in alcoholic solutions, are apt to be hard and wiry. When soaked in water they become weakened.

If, however, they are preserved in alcohol, to which a little glycerine has been added, or immersed in a solution of glycerine at the time of using, they become soft and pliable.

The final preservation of sutures is important, as the strength depends much upon it. Kyle tested the strength of gut kept in sublimated alcohol and juniper oil, and the effect of immersion in various solutions. Gut kept in sublimated alcohol was found weaker than that preserved in juniper oil; this was especially evident by comparison of the larger sizes of gut. The small sizes of gut were of about equal strength. Pure alcohol increases the strength of both preparations; alcohol and carbolic (1-40), equal parts had little effect on gut prepared in juniper oil, but increased the strength of that preserved in juniper oil and immersed 20 to 40 minutes in alcohol, and watery solutions of sublimate, were lessened considerably in strength. Immersion in boiled water weakens both preparations.

From Kyle's test it seems that absolute alcohol increases the strength of the gut, and is its best preservative. Juniper oil is next, while sublimated alcohol is of the least value. Carbolized oil as a preservative has been abandoned in continental Europe.

[30] Drousart devised an operation for the treatment of congenital ptosis, which consists of the formation of a tendon out of connective tissue deposited in the track of a catgut suture. Ordinary catgut was absorbed too quickly, 2 to 3 days, so he used that treated by naphthol which [31] Dehenne has found to last twelve to fifteen days in the tissues. Naphthol is used as a preservative, and in certain cases, as the above, may be of special value.

The most important animal tissues which are used for the manufacture of sutures and ligatures are catgut, tendons from the tail of the kangaroo, opossum, rats, squirrels, whales, rabbits and some other animals. Strips of fascia and certain skins, as buckskin, parchment and chamois leather, also hairs from the tail of the horse. Of these, catgut is the most important, and most universally employed, because it can be so readily obtained, and kangaroo tendon the most excellent, as it is stronger; does not swell in the tissue; from its nature it is more easily made aseptic, and can therefore be more depended upon; and as it resists absorption much longer than catgut prepared in the same way (ratio 1-3), a finer thread can be used when this is of advantage, and in regions, as the cervix, where it is desirable the sutures should hold for a somewhat longer period than usual, the same size suture of tendon will remain firm several days after a suture of catgut has become absorbed. The great objection to kangaroo tendons is the difficulty with which they are obtained, and their cost.

In regard to tendons derived from other animals, many may be easily obtained and utilized to great advantage in all branches of surgery. [32] Dr. E. O. Belt has made extensive use of sutures derived from the tail of rats in ophthalmic practice. The tail is skinned, and soaked in water for several days, when on slight manipulation, it splits into perhaps a hundred fibres, each about 8 inches long. They are placed in alcohol, and about once a month, for two or three days at a time, they are soaked in a 1–5,000 solution of corrosive sublimate. Dr. Belt recommends these fibres in cases where a strong and fine animal suture is required. He says they are much finer than those prepared from the opossum's tail, which he has seen used by Dr. Chisholm.

The fine tendons in the tail of our common rodents are very strong, but they are too short to be used extensively as a continuous suture; however, they can be used as an interrupted suture and as a ligature. I have used the fine tendons from the tail of a gray squirrel to close wounds on the face, where it was desirable to avoid scar formation as much as possible. These tendons are as fine as the finest silk, and are very strong.

Discussion.

Dr. George Jones remarked that in the future the animal ligature would be in the greatest demand. He prepares the ligatures according to Lister's method. Whenever suppuration occurred in his practice he had always been able to discover that in their preparation sufficient precaution against sepsis had not been taken. Much credit is due Dr. Marcy for the persistent manner in which he has urged the use of the animal suture.

Dr. Jones stated that quite recently he had boiled catgut in refined petroleum. Dr. W. S. Brown prefers silk ligature. He uses the black iron-dyed because the white contains lead. He objects to the use of corrosive sublimate in the preparation of sutures. He also stated that strong alcoholic solutions would do less harm than dilute watery ones.

Dr. Burt prepares gut by Lister's method, diluting the chromic acid solution to 1–6,000, and determines by the color when he has effected this. He keeps the ligature dry, and has never had any trouble from gut preserved in this manner. He prefers the rough material used by clockmakers, because it is of more uniform strength. In the preparation of violin strings, the gut is sandpapered to make it smooth. This treatment renders it weak and unreliable for surgical uses. In operations for the repair of the cervix he prefers silk ligatures.

Dr. Clarke, the President, endorsed the use of the animal suture, and especially the ligature made from the kangaroo as prepared by Dr. Marcy. The speaker had been associated with some of Dr. Marcy's earliest cases, and could bear witness as to the superiority of this method. He further said that the silkworm suture, however carefully prepared, was not at all comparable to that made from the kangaroo. Speaking in reference to the use of the silver suture, he said that its employment often required a second operation which was dreaded by the patient much more than the primary. In his own practice he had never seen any untoward results from the use of animal suture.

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The American Health Resort Association.

This Association met at the Tremont House, Chicago, June 30, and held three sessions.

There were present delegates representing Canada, Michigan, Massachusetts, Wisconsin, Florida, New Hampshire, New York, Pennsylvania, California, Illinois, Vermont, Colarado, Texas, Iowa, New Mexico and Central America.

A large correspondence was read by W. A. Chatterton, Secretary, from the absent members in various parts of the country.

The President, T. C. Duncan, M.D., of Chicago, then delivered a lengthy address, in which he outlined the good work of the Association, and how it was appreciated by the profession, enabling them to select climates adapted for the various cases of consumption. From reports received from the winter points, New Mexico had proven the most satisfactory This is of interest to the profession who are trying to save some of the "hundred thousand consumptives" who die annually in this country.

Dr. J. F. Danter, of Toronto, Canada, read a paper on the "Climates and Resorts of British America."

A report on the climate of Manitoba was read, from Dr. Clark, of Winnipeg.

The climate of New Brunswick was presented by Dr. J. Z. Currie.

From these reports it seems that there are a large number of consumptives in Canada, especially in the eastern provinces.

Dr. Adam Miller read a paper on sun spots and magnetic influence in disease.

The climate of Nebraska was presented by Dr. Brown.

The climate of California and its resorts was presented in papers by Drs. J. D. Hartley and S. W. Andrews, of Chicago.

Dr. W. P. Roberts, of Boston, read a report on the climate of New England, in which he reported that 15,000 die annually there from consumption.

"Consumption in Michigan" was the subject of a paper by Dr. Veenboer.

Dr. O. W. Gordon, of Council Bluffs, reported his disappointment in visiting various resorts, and spoke highly of New Mexico.

A report from Dr. A. Petin, of Las Cruces, N. M., formerly of Paris, was read in which he said they had almost constant sunshine, less than 2 inches of precipitation in 28 months, and that consumptives sent there were all doing well.

A report on the Adirondack region was read from Dr. Skinner.

Dr. B. W. James, of Philadelphia, contributed a paper on "Climate Maxims."

The climate of Costa Rica was presented by Dr. Buchanan.

Dr. A. S. Butler reported on the climate of Honduras.

"Texas as a Resort for Consumptives" was the title of a paper by Dr. Marshall.

Reports on mineral waters were presented from Las Vegas Hot Springs, N. M., Eureka Springs, Ojo Caliente Hot Springs, N. M., Costa Rica and Londonderry.

Prof. I. N. Danforth gave an address on "Mineral Waters, their Analyses and Uses."

He said the profession was being imposed upon by imperfect and fraudulent analyses. In the first stage of Bright's disease he thought that bland water should be used, and in the second Lithia waters.

Prof. W. S. Haines made a valuable report on Bacteria in Mineral and Potable Waters. In some mineral waters he found 2 bacteria to the cubic centimeter, and in some drinking water he found as high as 8,000.

A large number of members were admitted.

It was reported that a Congress of Climatologists would meet in Chicago next year, and it was voted that the Association meet with it.

The following officers were elected: T. C. Duncan, M.D., President, Chicago; J. F. Danter, M.D., first vice-President, Toronto, Canada; I. N. Danforth, M.D., second vice-President, Chicago; W. P. Roberts, M. D., third vice-President, Boston, Mass.; T. S. Hoyne, M.D., Treasurer, Chicago; W. A. Chatterton, Recording Secretary, Chicago; J. D. Hartley, M.D., Corresponding Secretary, Chicago; W. W. Van Baun, M.D., Philadelphia; Prof. W. S. Haines, M.D., Chicago.

The full proceedings and papers will be published shortly and all members will be supplied with these valuable and interesting transactions. For further particulars address

J. D. Hartley, M.D., *Corresponding Sec'y.*
1204 Milwaukee Ave., Chicago, Ill.

NECROLOGY.

Dr. James Sproat Green, of Elizabeth, New Jersey, died on the 1st, instant, aged 62 years. He was an alumnus of the University of Pennsylvania, class of 1851, and a member of this Association since 1872. After graduating he acted for two or three years as an assistant demonstrator of anatomy at his *alma mater*. He was on interne duty also, until 1853, at the Wills and Pennsylvania hospitals, after which time he took up his residence in Elizabeth. He gave special attention to orthopedics and abdominal surgery. He was repeatedly the president of the City Council of his city, and mayor for the year 1877. He was a founder of the Elizabeth

Hospital and a member of the surgical staff down to the time of his fatal attack, which was very sudden, by cardiac disease. He was on the point of going out in his carriage to attend a patient, when he was taken ill and in a few minutes had expired. He stood in the front rank of his profession in the northern part of his State. He had been for four years one of the board of managers of the Lunatic Asylum at Morris Plains. In 1890 he was chosen president of the New Jersey State Medical Society, an honor that befalls only the best and most worthy of the medical fraternity in that commonwealth.

DR. JAMES BARRON POTTER, of Bridgeton, New Jersey, died on shipboard while returning from Florida, on the 11th of June. He was a graduate of the University of Pennsylvania in 1847. He had passed the winter at his plantation near Fort Myers and finding himself to be failing in health, he determined to return to his home by the steamer from Jacksonville. He succumbed by disease of the heart, after the vessel had been about forty-eight hours out from that port. He was in his seventieth year.

NOTES.

AMETROPIA IN HORSES.—In London it has become a recognized branch of the optical business to supply spectacles for horses whose eyes are the seat of some error of refraction. It is claimed as a well authenticated observation that myopia is a prominent cause of the vice of shying, and the value of a good steed may be considerably enhanced by a cheap pair of concave glasses. But it is said that the principal use thus far made of horse spectacles is to make high-steppers of those animals whose walk in life is to be that of the hack or coach horse. While yet young the steed is given a pair of convex lenses set in stiff leather caps, entirely enclosing the orbit. The horse is misled thereby and sees the ground much higher than it really is; this has an effect upon his steppage that makes his motion admired and fashionable for cab and coupé purposes.

RARE FATALITIES AMONG PUBLIC MEN.—Prime Minister Honorable John Robson, of British Columbia, died in London recently in consequence of a seemingly trivial accident. The end of one of his fingers was caught and contused in the closing of a door of a hansom cab. The injured member was the little finger of his right hand. The injury was sufficiently severe to demand the amputation of the finger. Septic symptoms came on and a fatal issue could not be prevented despite the best of attendance. Mr. Robson was not in good health at the time of his arrival in London. Another gentleman, well known in the Conservative party in England, also died in the prime of life, namely Mr. Frank J. Woods, the secretary of one of the leaders in the House of Commons, Mr. Balfour. He died in ten days after being stung by an insect, supposed to be a gadfly. The injury was received upon the lip and was followed by an erysipelatous inflammation that resisted all efforts at cure. The attack may possibly have been one of malignant vesicle or charbon, of which our English cousins, fortunately, see comparatively little.

AN EMINENT LAWYER'S PORTRAIT OF HIS PHYSICIAN.—Hon. Thomas F. Bayard delivered the commencement address before the Baltimore College of Physicians and Surgeons, at its recent twentieth anniversary. His theme was to show the points at which the legal and the medical professions touch in the homes of misfortune and suffering, especially that affecting the mind rather than the body. In his long experience of life, he has witnessed that "brotherhood of the lawyer and doctor at a common fountain," that which never ceases flowing—the fountain of human sorrows and distress. Each profession has its function of cure, and the power of prevention. The necessities of modern social life make the physician the repository of a host of secrets, which become sacred to him because these secrets are essential to his full knowledge of his patient's requirements. In enlarging upon these opinions, Mr. Bayard becomes more specific and personal. He refers to his own circle of acquaintance, and sketches the portrait of a physician who has carried consolation as well as medicines into the homes of his Delaware circuit:

"The physician," he said, "who thus relieves sorrow and anxiety by receiving them and sharing them can make no proclamation of his well-doing or the service he has rendered, and if he ever hears an applauding voice, it is now and then, but not always, 'the still, small voice of gratitude.' I doubt if there were any real physicians among the sect called Pharisees. Luke was called 'the beloved physician,' and in his history I find no suggestion that Luke was a Pharisee.

"You will perceive that it is upon the duties and responsibilities of physicians and lawyers as citizens that my comments have been chiefly made, for I never knew a really great physician who was not greater as a man—I mean, whose greatness did not rest upon his personal and moral basis, which elevated and strengthened his professional life, infused itself into the community in which he lived, and was in fact the underlying and pervading cause of his influence and consequent success in his profession. *It has been my personal fortune to know such a man. It has been my privilege and delight to accompany him in visits where his only medicines were the personal presence and conversation of the man himself.* He had shared and had lessened their anxieties; counselled the wayward; had led the sick back to health; cheered the weak-hearted; had 'rejoiced with them that did rejoice and wept with them that wept.' And I have seen such a man so surrounded by an atmosphere of love and trust, holding as it were the heart-strings of a family in his hands, their 'guide, philosopher and friend,' and then I realized what a moral force in society the profession, properly comprehended and properly followed, was capable of exerting, and how relatively small a part of its usefulness was the administration of medicine."

PROPOSED BIOGRAPHY OF THE LATE DR. AGNEW.—Dr. J. Howe Adams, of Philadelphia, has been desired by Mrs. Agnew to prepare a memoir of the late great surgeon, and the friends far and wide who can contribute unpublished incidents, stories, anecdotes, sayings and the like, will confer a favor by promptly sending their quota forward to Dr. Adams. In this respect, as in so many other situations in life, *qui cito dat bis dat.* To be timely and at its best, a memorial of this character should not be allowed to linger in its composition. Those who were his students in the fifties and sixties, and before the *Index Medicus* began to catalogue every bit of printed work a man does, will be most likely to furnish materials that are not already pretty well known to Dr. Adams. About twenty years ago, if we mistake not, Dr Agnew delivered an address before his class at the University, subject "Error in Diagnosis," about which his friends rallied him not a little, on the ground that all the errors for which confession was explicitly made were the errors committed by others, either his colleagues or his forefathers in surgery. He freely acknowledged his own fallibility in general terms, but the illustrative instances, as his friends chided him, were postponed for some other occasion or some other author. The address was a charming and informing one "all the same." It was on that occasion that Dr. Agnew took occasion to refer to the unseemly alacrity displayed by some practitioners to serve as expert witnesses against their fellows, in suits for malpractice. Dr. Agnew did not contend that all surgeons should be shielded, whether right or wrong, but he did maintain that before one practitioner testified against his brother, the former should have made himself conversant with every salient feature of the case in dispute. The man who cannot or will not do this much is unworthy of his place in the profession, and is liable to become a mere breeder of mischief. Dr. Agnew's influence, in this direction, was undeniably wholesome and just, and we trust that Dr. Adams will be able to illustrate it fully and strongly.

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SATURDAY, JULY 16, 1892.

BY-LAW IV OF THE AMERICAN MEDICAL ASSOCIATION.

The Publication of Papers and Reports.

No report or other paper shall be entitled to publication in the volume for the year in which it shall be presented to the Association, unless it be placed in the hands of the Board of Trustees on or before the first day of July. It must also be so prepared as to require no material alteration or addition at the hands of its author.

Authors of papers are required to return their proofs within two weeks after their reception; otherwise they will be passed over and omitted from the volume.

Every paper received by this Association and ordered to be published, and all plates or other means of illustration, shall be considered the exclusive property of the Association, and shall be published and sold for the exclusive benefit of the Association.

The Board of Trustees shall have full discretionary power to omit from the published *Transactions*, in part or in whole, any paper that may be referred to it by the Association, or either of the Sections, unless specially instructed to the contrary by vote of the Association.

BRONCHO-PNEUMONIA OF INTESTINAL ORIGIN.

Five years ago SEVESTRE, from clinical observation alone, became convinced that broncho-pneumonia in infancy was occasionally of intestinal origin, and concluded one of his papers in the following language:

"1. In children one or two years of age (and probably also of other ages), subjected to a vicious alimentation, there may occur a decomposition of the intestinal contents, resulting in a fetid diarrhœa and an infectious enteritis.

2. General infection may follow, and particularly pulmonary congestion and broncho-pneumonia.

3. Intestinal disinfectants, especially calomel and naphthaline, are the best means of relieving the diarrhœa, and of preventing pulmonary manifestations."

The work of his pupil, LESAGE, we reviewed a few weeks ago. More recently, two others of his pupils, GASTON and RENARD [1] have contributed the results of their work in the same field.

The chief characteristic of the trouble is the occurrence of the pulmonary disorder as a complication in the course of an existing diarrhœa. The cases usually occur in infants who have been badly fed. After more or less prolonged dyspepsia, diarrhœa sets in and is characterized by frequent stools, green, white, or yellowish and very putrid. In from two to six days broncho-pneumonia appears, and is ushered in by more or less fever, by cough and dyspnœa. Very young infants lie quietly upon the back or side with the thighs flexed upon the abdomen. While in older children some interest in surroundings is manifested. Often the abdomen is distended with gas and is sensitive to palpation. Gurgling occurs in the right iliac region, and the skin is flaccid and non-elastic. The tongue is white, covered with a thick grayish coating in the center, and red on the edges. The stomach is dilated with gas, and the spleen is often perceptible on palpation. Fever, which is sometimes wanting, varies between 102 and 104 and is very variable. The cough varies a great deal, and the dyspnœa, not usually intense, is shown by respirations varying from 20 to 45 per minute. The physical signs show broncho-pneumonia, but vary greatly from day to day. The disease lasts usually from one to two weeks, but may be prolonged to two months.

The authors describe four forms of the trouble: The supra-acute or choleric form; the grave or typhoid form; the prolonged or remittent form; and the light or common form. The first form is rapid and terminates in two or three days, the patient dying in an algid stage. The typhoid form lasts two or three weeks, has a quite constant temperature and rather less diarrhœa. In the remittent form, which SEVESTRE has described very carefully, the temperature remains elevated for two or three days, and then drops for a like period, when it again rises. These exacerbations and remissions may occur for four or six weeks, and each exacerbation is accompanied by a new patch of broncho-pneumonia. The diagnosis is not usually difficult. The appearance of fever and cough in the course of a fetid diarrhœa, in a badly fed or bottle fed baby under two years of age, should raise the suspicion of a broncho-pneumonia of intestinal origin. Generalized bronchitis gives less fever, and no diarrhœa. Frank pneumonia is relatively rare, and should be made out by the physical signs. Pneumonia complicating acute infectious diseases is readily differentiated by the accompanying conditions. Typhoid fever is difficult to distinguish; attention should be given to the rose spots and, in the authors' opinion to the temperature curve. Tuberculous broncho-pneumonia it may be impossible to differentiate.

[1] Rev. Mens. des Mal. de l'enfance. May, 1892.

Some interesting pathological work on this subject has already been done. LUBARSCH and TSUTSUI, in a case of diarrhœa complicated with pneumonia, have found, in both the intestine and the lung, the *bacillus enteridis* of GOERTNER. GILBERT and GIRODE in a case of cholera nostras have found the *bacillus coli communis*, associated with other forms, during life. CHANTEMESSE and WIDAL in six cases of typhoid fever have found the *Eberth bacillus* in broncho-pneumonia. LESAGE on the contrary asserts that pulmonary lesions complicating enteritis are always due to the presence of the *bacillus coli* in the lungs.

The pathological observations of the authors were made upon twenty-six cases of broncho-pneumonia with infectious diarrhœa. They made cultures from the stools and also from the pulmonary juice, which they obtained by punctures made into a pneumonic spot, during life.

The stools, naturally, gave various forms; among them were observed the bacillus coli communis, bacillus subtilis, bacterium termo, bacillus enteritis of Goertner, and diplococci. Of the 26 broncho-pneumonias examined, 16 gave only a single species of microörganism, 5 showed several species, and from 5 no cultures were obtained. Where pure cultures were obtained they were as follows:

Pneumococcus,	8	times.
Staphylococcus,	3	"
Bacillus coli,	3	"
Encapsulated bacillus,	2	"

The other five cases showed:

Pneumococcus and staphylococcus,	3	times.
Staphylococcus and bacillus coli,	1	"
Pneumococcus and bacterium termo,	1	"

From these observations the authors conclude, 1. That in the majority of cases, broncho-pneumonias occurring in the course of the infectious diarrhœas of infancy, are secondary, and are due to superadded microörganisms, and (in some cases only) are due to the specific agent of the infectious diarrhœa. 2. That in the majority of cases, the specific agent of the diarrhœa, favors by its secretions, the virulence of the parasitic species normally contained in the buccal cavity, and renders them pathogenic.

From further experiments on lower animals, which space prevents our reviewing here, the authors hold, that the bacillus coli is capable of establishing a true septicæmia, which in the infant starts in the intestine, and is provoked by diarrhœa. The bacillus coli passes from the intestine to the lungs by way of the lymphatics and blood vessels, and in this way produces the lesions of these organs.

In the lungs the ordinary lesions of broncho-pneumonia were found and in other organs generally, congestions with tumefactions, or granulo-fatty degeneration of the cellular elements, such as are commonly met in acute septicæmias.

Such observations as the foregoing are very valuable, and exceedingly suggestive. They seem to open the way to a more rational handling of a certain class of broncho-pneumonias. If clinical experience counts for aught, it would seem that the pulmonary lesions of intestinal origin are not limited to pneumonia, but that acute bronchitis is not infrequently traceable to a similar origin.

This work is particularly interesting in connection with the subject of typhoid fever in infancy.

That typhoid fever occurs more frequently in infants than was formerly supposed is coming more and more to be admitted. When it does occur the bronchial symptoms are usually exaggerated, and pneumonia sometimes supervenes. While typhoid in the infant can not be recognized clinically in a given case unless the peculiar eruption has appeared, it would be going too far to say that the absence of the eruption proves the absence of typhoid.

The appearance of the tongue which authors have described, is exactly that found in typhoid fever in the infant, and their grave or typhoid form is highly suggestive of the so-called enteric fever. Again, the relation of the bacillus coli to these cases furnishes food for conjecture, for RODET and ROUX still claim that the Eberth bacillus is but a modification of the bacillus coli.

THE "BREATHING" HOSPITAL.

How to make a hospital, or other public edifice where fresh air is a prime desideratum, renovate its atmosphere with proper rapidity is a question which is being answered better and better each year. The *Medical Press*, in treating of some recent devices, says that considerable gains have been made at the numerous public buildings where large fans have been put in, "but the results are generally less satisfactory than those obtained in industrial buildings, mainly because the ventilating engineer is not allowed to handle a public building *so as to make it breathe in the same free way* that is easily permitted in a factory, where æsthetic and other nice points can be left to take care of themselves." This is a most happy expression regarding the free full breath which such structures as hospitals should be competent to take, and take again, at definite respiratory intervals. One of the latest devices that has come up for examination and trial is one in use at Victoria Infirmary of Glasgow, the operation of which is to sift, and at the same time dampen, the inspired atmosphere before it comes into contact with the heating coils. Here we have an analogue of nasal breathing in the human subject, which is admittedly the normal, and as compared with "mouth-breathing" the less dangerous sort of inspiratory act. The nose-breathing of public buildings is contrived in this manner: The air is renewed six times in each hour; before it enters the wards it is filtered and washed by being passed

through an air-washing screen of cords, formed of horse hair and hemp, closely wound over wooden rails at top and bottom, forming a close screen sixteen feet long by twelve feet wide or high, affording nearly two hundred square feet of artificial Schneiderian surface. There is a constant trickling of water down and over this screen, rendering it wet at all times, thus aiding in the detention of all dust and soot that would otherwise pass through into the wards. When once these filtered particles have adhered to the wetted cords, a current of air of considerable violence is not competent to carry them through the screen, but the dripping water carries them downwards into the sewer. An additional cleansing of the screen is provided for in the use of the following neat contrivance: An automatic flushing tank, of twenty gallons capacity, is fixed at the head of the screen and timed so as to empty itself once an hour, night and day, and flush away all particles that tend to clog the inlet. The Infirmary is located in a wholesome section of the city, near to the Queen's Park, yet the screen entangles a very considerable number of soot particles from the supplied air. One of the properties of this device is said to be the clearing up of the admitted air in times of foggy weather. Last winter when there were many days of dense fog, the air within the wards seemed peculiarly bright and clean. This nearly rounds out the suggestive simile of the "nose-breathing" system of ventilation, except that the air is warmed before it advances much further—and this done by coming into contact with steam-heated coils. But this is so self-evidently physiological and so wanting in novelty that no further comparisons are needed.

The ducts for the admission of the heated purified air are wide and shallow, cut into the wall about five feet above the floor; their direction is upward. The outlets are at the floor-level. Some analysis of the air, taken at three feet below the ceiling, discovered no organic matter, and the air of the ward generally was exceptionally free from microbes and molds.

The editor of the *Press* suggests that the time may come when the air will be cooled, by the use of ice, during the heated term, as well as warmed in winter as is now done. The claim is made that the patients are absolutely freed from exposure to draughts. Another suggestion is made that fever hospitals, and all places where small-pox and communicable diseases are treated, may so arrange their exit air-ducts that all the expired air shall be burned, or passed through fire, on its way into the outer world. There is at least one organized ventilating company in England which professes to be able to carry out in a practical way the above theories that our public buildings can be made to breathe in a vivifying and purifying manner.

OSTEOPLASTIC THERAPY OF SPINA BIFIDA.

The occurrance of a *spina bifida* without other deformity has been placed (WERNITZ) at one in every thousand births. This makes this error of development a conspicuous object of therapy, not only on account of the interest of the parents, but from the quantitative value of the patients. Up to the present time, very poor results have followed except where the boney defect happened to be small. The obliteration of the meningocele has too often been followed by a return of the tumor. This return is due to the stretching and giving way of the muscular and connective-tissue covering of the cord, and these returns give the indication for successful treatment.

The treatment of defects in the spinal arch by an osteoplastic operation was proposed in 1885 and successfully carried out in one case by DOLLINGER. He divided and brought together the rudiments of the spinal arches in the lumbo-sacral region and succeeded in covering in an exposed cord. Since that time a few cases have been reported at considerable intervals. In a recent number of the *Centralblatt für Chirurgie*, BOBROFF[1] of Moscow, reports a very significant operation with a remarkably favorable result. The patient, an eight year old boy, had a considerable defect in the lumbo-sacral arch, and a myelomeningocele as large as a base-ball. It could be depressed so that half the fluid disappeared, when the child complained of pain and syncope. There had been complete incontinence of urine and feces since birth. After the ordinary method of reducing the sack had been accomplished through two lateral openings, a flap of bone with the overlying muscle was raised from the remnants of the arch. This flap was about three centimetres square on each side; with these two flaps the defect was covered and the pieces held in place by bone-sutures. There was union everywhere in a few days and after three months the patient began to control the sphincters.

This happy example of rational treatment should stimulate surgeons to resort to the osteoplastic operations in all suitable cases. It is necessary to remember that most defects in the mural arch are accompanied by defects in the cord itself, and that in order to be successful the surgeon must understand, as an embryologist, the exact conditions under which the defect or arrest of development occurred. In the majority of cases the meningocele will be found continuous with the central canal of the cord and the *cauda equini* will be ribbed out in the cyst wall. The cord itself will be found deformed and unequally developed, and the remnants of teratoids must be looked for. There are, doubtless, cases where the defect in the bony arch is so small that an osteoplastic operation is unnecessary. There are cases in which

[1] A. A. Bobroff. Ein neues osteoplastisches Verfahren bei Spina bifida, No. 22 p. 465—467.

the defect is so great and is accompanied by such extensive defects in the cord that a successful restoration of the mural arch would leave the patient in a pitiable condition still; but between these two extremes there is certainly a wide field for the procedure.

The indications for treatment in these cases are the more imperative from the serious effects which the wearing of a meningocele has on the nervous system, and the uniformly progressive nature of the defect itself. The child's life should not be endangered by the lesser operation, when hope of permanent recovery lies only in the restoration of the bony arch. The child's life should not be endangered by months or years of waiting, because they are months or years of danger of rupture and infection while the bones are becoming more and more difficult to manipulate and the child is growing dearer and dearer to its parents.

CONFERENCE OF STATE MEDICAL EXAMINING AND LICENSING BOARDS.—The Preliminary Conference of the several State Medical Examining and Licensing Boards of the United States, which was held in Washington, D. C., during May, 1891, demonstrated the utility of such Conferences, and resulted, during the session of the American Medical Association last month in Detroit, in the formation of an organization for the purpose of holding Annual Conferences of the Member and ex-Members of each State Medical Examining and Licensing Board of the Union having such a Board.

The meeting in Detroit elected Dr. John H. Rauch, of Springfield, Ill., President; Dr. Wm. W. Potter of Buffalo, N. Y., Vice-President; and Dr. Hugh M. Taylor, of Richmond, Va., Secretary and Treasurer.

As stated in the constitution, the objects of this organization are to elevate the moral and mental tone of the medical profession, to divorce the medical licensing from the teaching powers, to encourage the establishment of medical examining and licensing boards, to secure harmony of action throughout the Union by the interchange of thought and experience, and to attain, as far as practicable, a uniformity of requirements for practice in the several States.

The Annual Meetings are to be held during the second day and at the place of meeting of the American Medical Association. Active and ex-members of State Medical Examining and Licensing Boards are eligible to membership. No action of the Conference is in any way binding upon the respective boards through the members who may participate in the Conference—the sole mission of the Conference being the diffusion of knowledge relative to the work of examining and licensing boards, and no Board as a board is represented or committed; but the individual active and ex-members participate in the meetings and give and receive information which will help them in their work.

Members and ex-members of examining and licensing boards who have not already connected themselves with this work are cordially invited to signify to the officers their willingness to do so. Such an organization is capable of accomplishing great good. But, in order that its mission may be carried to its highest possible end, it is necessary to have the active coöperation of many representatives from all the boards.

THE SURGEON GENERAL OF ILLINOIS.—We hear and read much in these degenerate days, of the improvement and purification of the Public Service, but it has been reserved for the present Governor of Illinois to show the estimate placed upon professional ability and experience by a thorough going politician. The telegraph informs us that a young graduate of the Hahnemann class of 1887 has been appointed Surgeon General by his Excellency, Governor Fifer, to fill the vacancy caused by the death of General Matthews. The action of the Governor in selecting this inexperienced young person for so important a position is inexplicable on ordinary grounds, but in common with many others, we are of the opinion that the Governor's sincere desire to secure the "finest military medical talent" in the State for Surgeon General, will receive at the polls in November that rebuke at the hands of the profession, which such misdirected action fully warrants.

ERRATA.—In Dr. Billings' health report, on page 25 of THE JOURNAL of July 2, the number of guests at hotels in St. Augustine should read 16,000 instead of 1,600.

THE BARNES HOSPITAL AND MEDICAL COLLEGE OF ST. LOUIS.—We are informed that the large bequest of Mr. Barnes was for the erection and endowment of a hospital, and was without provision for a college. However, a practical connection of the two institutions is designed, and it is hoped by the friends of the latter to secure all needed funds to make it a first class institution of medical learning.

OFFICIAL LIST OF CHANGES in the Stations and Duties of Officers Serving in the Medical Department, U. S. Army, from July 2, 1892, to July 8, 1892.

Lieut.-Col. William D. Wolverton, Asst. Medical Purveyor, will, on the expiration of his present leave of absence, proceed to Ft. Omaha, Neb, and report in person to the commanding officer of that post for temporary duty during the absence of Major Albert Hartsuff, Surgeon, on leave.

First Lieut Isaac P. Ware, Asst. Surgeon U. S. A., is relieved from duty at Ft. Douglas, Utah, and will report in person to the commanding officer at Ft. Logan, Col., for duty at that post, relieving Capt. William L. Kneedler, Asst. Surgeon U. S. A. Capt. Kneedler, on being relieved, will report in person to the commanding officer, Ft. Mason, Cal., for duty at that station, relieving Capt. Harry O. Perley, Asst. Surgeon.

Capt. Harry O. Perley, Asst. Surgeon, on being relieved from duty at Ft. Mason, Cal., will repair to Washington, D. C., and report in person to the Surgeon-General U. S. A., for duty in his office.

Capt. William Stephenson, Asst. Surgeon U. S. A, will visit the camp of the Illinois National Guard at Springfield, Ill., during the period of its encampment, commencing July 9, and ending August 21, 1892.

Capt. William H. Corbusier, Asst. Surgeon U. S. A., will proceed, at the proper time, to Island Lake, Mich., and visit the camp of the Michigan State Troops, during the period of their encampment at that place, commencing August 18, 1892.

Capt. Henry S. T. Harris, Asst. Surgeon U. S. A., leave of absence granted is further extended one month.

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(DR. FOTHERGILL.)

℞—Pulv. Ipecac . . . . . ⅔ gr. | Strychnine . . . . . 1-20 gr.
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